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TRANSACTIONS

OF THE

Royal Academy of Medicine

IN IRELAND.

KING'S COLLEGE HOSPITAL
MEDICAL SCHOOL

VOL. VII.

EDITED BY

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MEDICAL SCHOOL

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RULES.

1. The name shall be, "ROYAL ACADEMY OF MEDICINE IN IRELAND." (1887.)

Constitution.

2. The Academy shall consist of Fellows, Honorary Fellows, Members, and Student Associates.

Management.

3. The affairs shall be managed by a Council, consisting of the President, the six Presidents of Sections, the General Secretary and Treasurer, the Secretary for Foreign Correspondence, six Secretaries of Sections, and eight Councillors, being two representatives from the Medical, Surgical, Obstetrical, and Pathological Sectional Councils respectively.

Meetings.

4. The Meetings shall be General and Ordinary.

Publication of "Transactions."

5. The "Transactions" shall be published by the Council, subject to the provisions hereinafter contained.

Original Fellows and Members.

6. All the Members of the present Societies (Medical, Surgical, Obstetrical, and Pathological) shall be Original Fellows or Members, without entrance fee, on payment of the annual subscription on or before 31st December, 1882.[a]

Fellows.

7. Fellows of the King and Queen's College of Physicians in Ireland, and of the Royal College of Surgeons in Ireland, shall be admitted, without ballot, on payment of the entrance fee and the subscription for the current year. All others, being Registered Medical Practitioners not directly or indirectly engaged in the sale of drugs, shall be proposed by two Fellows, and elected by ballot by the Council.

8. Candidates shall be proposed at one Meeting of the Council, and balloted for at the next—one black bean in four to reject.

Privileges of Fellows.

9. Fellows only shall be eligible for office in the Academy. They shall have the privilege of attending all Meetings of the Academy, of making Communications, and of voting and speaking at such meetings. They shall also receive a copy of the "Transactions."

[a] Those who have paid a Life Subscription to any of the above Societies will be admitted to the privilege of Fellows on payment of Member's subscription.

10. These privileges shall not be exercised by any Fellow in arrear with his subscription.

Honorary Fellows.

11. Honorary Fellows, limited in number to 25, may be nominated by the Council, and elected, on motion at a General Meeting of the Academy by a majority of at least two-thirds of those present and voting.

Members.

12. Any Registered Medical Practitioner may be elected as a Member, the election to be conducted in the same manner as that of Fellows.

Privileges of Members.

13. Members shall have the privilege of attending the Ordinary Meetings of the Academy, of making Communications, and of taking part in debate. They can purchase the "Transactions" at cost price.

Student Associates.

14. Registered Medical Students, of the third or subsequent years, may be elected as Student Associates in the same manner as the Members.

15. Student Associates shall have the privilege of attending the Ordinary Meetings of the Academy.

Annual Subscription.

16. Fellows shall pay £2 2s., and Members £1 1s. Student Associates shall pay 5s. The Subscription shall become due on the 1st of October in each year, and if the Subscription be not paid on or before the first Meeting in February, the defaulter shall cease to belong to the Academy, unless the delay shall be accounted for to the satisfaction of the Council. No Fellow shall vote at the Annual General Meeting who has not paid his subscription for the year. Medical Officers of the Army and Navy, and Registered Medical Practitioners not residing within 15 miles of Dublin, are eligible as Fellows of the Academy on payment of the entrance fee, and an annual Subscription of £1 1s.

Entrance Fee.

17. After admission of Original Fellows, all Fellows shall pay an entrance fee of £1 1s.

Council.

18. The Council shall meet on the first Wednesday in the month throughout the Session, or oftener should they see occasion—five to form a quorum.

19. Notice of all Extraordinary Meetings shall be transmitted by the Secretary to every Member of the Council. The President or any five Members of Council may call an Extraordinary Meeting of the Council. The Council shall determine questions by vote, or by division if so demanded, the President having a casting vote only. Any regulation of the Council shall have the force of a law, until submitted to the next General Meeting. The Council shall have the power of filling up any vacancies which may occur in the list of Officers of the Academy, except that of President, before the Annual General Meeting. If a vacancy in the office of President should occur, the General Council shall summon a Special General Meeting of the Academy to fill such vacancy. (1888.)

Sectional Councils.

20. There shall be six Sectional Councils elected by the Annual General Meeting in October, termed respectively—the Medical, the Surgical, the Obstetrical, and the Pathological, the State Medicine, and the Anatomical and Physiological Councils.

21. No Fellow shall be eligible as a candidate for election on more than two Sectional Councils, but no Fellow shall be eligible as a candidate for election on both the Medical and Surgical Sectional Councils. (1888.)

22. Each Sectional Council shall consist of the President of the Section and ten Members, one of whom shall act as Secretary to the Section ; except the State Medicine and Anatomical and Physiological Councils, which shall each consist of a President and six Members. (1888).

Meetings of Sectional Councils.

23. Each Sectional Council shall meet on a fixed day at least one week before the Ordinary Meeting of their Section, three to form a quorum.

Powers.

24. Each Sectional Council shall have the power of making any such arrangements as it thinks necessary to carry on the work of the Ordinary Meetings which are under its charge, provided that such arrangements do not interfere with the general laws of the Academy ; and any Rules laid down by such Council shall have the force of laws at the Ordinary Meetings under its charge, until submitted to the General Council.

25. Each Sectional Council shall have the power of filling up any vacancies that may occur among its Members until the Annual General Meeting.

Committee of Reference.

26. The Council shall appoint a Committee of Reference, to report upon morbid growths and other specimens exhibited before the Academy ; of this Committee the Exhibitor shall, for the occasion, be a Member.

Officers.

27. A President, to be elected by the Annual General Meeting in October, and to hold office for three years.

28. The Presidents of the Colleges of Physicians and Surgeons for the time being shall be the Presidents of the Medical and Surgical Sections. The Presidents of the other Sections shall be elected by the Fellows at the Annual General Meeting, and shall hold office for two years. (1888.)

29. One General Secretary and Treasurer to be elected at the Annual General Meeting.

30. It is expedient that a fixed salary (of one hundred guineas) shall be paid yearly to the General Secretary in consideration of the fact that the editing of the "Transactions" is part of his duties.

31. One Honorary Secretary for Foreign Correspondence to be elected at the Annual General Meeting. (1888.)

32. The Councillors for each Section to be elected at the Annual General Meeting. Each Sectional Council shall elect two Members to act on the General Council, except in the case of the Sections of State Medicine and Anatomy and Physiology. (1888.)

33. Two Members in each Sectional Council shall retire annually, and be ineligible for re-election for one year.

34. Six Secretaries, one for each Section, to be appointed by the Sectional Councils.

35. At all elections after the year 1882, any Fellow desirous of nominating a candidate for election on any of the Sectional Councils shall, at least one fortnight before the Annual General Meeting, forward an application to the General Secretary to enter the name of such Fellow on the list of candidates for the office of Councillor of that Section, provided that the Fellow so nominated shall have consented to act. (1886.)

36. That all elections shall be by ballot.

Duties of Officers.

37. *The President* shall preside at the Annual and Special General Meetings and at General Council Meetings. In the absence of the President, the Chairman shall be appointed by the meeting. (1888.)

38. *The Presidents of Sections* shall preside at the Ordinary Meetings of the Academy, and shall also preside at the Sectional Council Meetings. In the absence of the President, the Chairman shall be appointed by the Meeting. (1888).

39. *The General Secretary* shall attend all General Meetings of the Academy and General Council. He shall take minutes of such meetings, to be read at the following meeting.

40. He shall receive and have charge of all papers intended for publication in the "Transactions" of the Academy, after they have been handed over to him by the Secretaries of the several Sections.

41. He shall, on receiving notice from the Secretary of a Section, send out to all the Members notices of the title or titles of the paper or papers for the next Ordinary Meeting, with the name or names of the authors, and, so far as possible, of the subjects for Exhibition, with the names of the Exhibitors.

42. He shall arrange for the Exhibition of specimens and the reading of papers, which are forwarded to the Academy by those who are absent, or are not members.

43. The General Secretary and Treasurer shall receive all moneys, and lodge the same in bank to the account of the Academy, and all cheques shall be signed by the Treasurer and one other Councillor.

44. The Accounts shall be audited by two Fellows, not Members of Council, to be appointed by the President at some meeting previous to the Annual Meeting.

Duties of Secretaries of Sections.

45. To attend the Meetings of the Council of the Section and the Ordinary Meetings of the Academy, under the management of said Council, and to take minutes at such meetings, to be read at the next following meeting of that Section.

46. To keep such papers as the Sectional Councils deem worthy of publication, for the purpose of handing them over to the General Secretary.

47. To inform the Secretary of the Committee of Reference of any specimens referred to that Committee, and to transfer the specimens to that Secretary.

48. To give notice to the General Secretary, one week previously to the meeting, of the titles of papers for the evening, the names of the authors, and, so far as possible, the objects for Exhibition, with the names of Exhibitors, so that the General Secretary may inform the Members.

Meetings.

49. The Annual General Meeting to take place on the last Friday in October, for the election of Officers and Members of Council, and for the general business of the Academy.

50. Due notice of the meeting shall be given by the Secretary to all members one fortnight previously, and the names of Candidates proposed for office must be in the hands of the Secretary one week before the meeting.

51. No motion involving a change of these Rules shall be brought before this meeting except one week's notice thereof shall have been given by the Secretary to each Member.

52. The President may—and shall forthwith, on receiving a requisition signed by seven Fellows, at any time—on giving one week's notice, summon a Special General Meeting, for the consideration of particular business, the nature of which must be specified in the letter of summons convening the meeting, and at such meeting no other business can be transacted. In the event of the President being unable, from any cause, or declining, to summon a Special General Meeting of the Academy, it shall be in the power of the General Council to summon such meeting. (1888.)

Ordinary Meetings.

53. The communications to be submitted to the Ordinary Meetings shall be grouped under the following heads :—Medicine, Surgery, Pathology, Obstetrics, State Medicine, and Anatomy and Physiology ; and the conduct of such meetings shall be in the hands of the several Sectional Councils, each Sectional Council to have the management of the Ordinary Meeting in rotation, as arranged by the General Council. (1888.)

54. The Ordinary Meetings shall be held on every Friday evening, from the first Friday in November until the last Friday in May, inclusive, at eight o'clock, except during the Christmas and Easter recesses.

55. All Fellows, Members, and Student Associates attending the meetings, shall write their names in the attendance book.

56. Any Fellow or Member may introduce two Visitors by cards obtained from the Sectional Secretaries.

57. Officers of the Army or Navy Medical Departments shall, on presenting their cards, be admitted to the Ordinary Meetings of the Academy.

58. No communication shall exceed twenty minutes in its delivery, nor any speech thereon ten minutes, except by permission of the Chairman. No one shall speak twice upon the same communication, except the author, who has the right of reply.

Ordinary Meetings.—Order of Business.

59. (1.) Chair to be taken at 8 30 p.m.

(2.) Chairman to read list of specimens, &c., exhibited by card, together with the names of the Exhibitors.

(3.) No Pathological Specimen shall be exhibited at any Section other than the Pathological and Obstetrical, except by card. This Exhibition shall not exclude any subsequent communication regarding it at the Pathological Section.

(4.) There shall be no Exhibition of Specimens by card in the Obstetrical or Pathological Sections.

(5.) Any member shall have liberty to exhibit any recent specimen at any of the meetings of the Obstetrical Section, provided it illustrates any question in gynæcology.

(6.) At the meetings of the Obstetrical Section recent specimens may be exhibited, and the President shall invite discussion thereon, provided that such exhibition of specimens or discussion, if any, thereon, must terminate at 9 o'clock, p.m., but that, if necessary, they may be resumed after the papers for the evening have been read and discussed.

(7.) Chairman to ask if any member has any observations to make or motion to propose relative to any living specimen on the List of Exhibition.

(8.) Chairman to call upon the author of the first paper on the list to read his paper.

(9.) Chairman to call upon members to discuss the paper, or, at his discretion, to take any other paper or papers on the list relating to the subject, and have the discussion subsequently on all such papers collectively.

(10.) When the last paper has been discussed, the Chairman to ask if any member desires to speak upon any of the specimens exhibited by card.

(11.) After the discussion upon any specimen, the Exhibitor has the right of reply.

Regulations regarding the Exhibition of Specimens by Card.

60. (1.) Any member may exhibit by card at any Ordinary Meeting, except at the meeting of the Pathological and Obstetrical Sections. At the meetings of the Pathological all specimens must be presented and described *viva voce*, and debate may be invited thereon.

(2.) Notice shall, if possible, be given to the General Secretary, or the Secretary of the Section, on or before the previous Ordinary Meeting.

(3.) Specimens must be in the room at 7 45 on the night of Exhibition.

(4.) Specimens for Exhibition by card shall be open for inspection at 8 p.m.

(5.) A card, containing all particulars for publication, shall be placed with the Specimen. Cards for this purpose are to be obtained from the Secretary.

(6.) The Exhibitor should be present, and he shall furnish further details if asked for.

(7.) Every Exhibitor shall submit the Specimen or Specimens on view to the Committee of Reference, if the meeting so decide.

Exhibition of Pathological Specimens.

61. No lengthened reference to treatment shall be allowed upon any Specimen, except by the express permission of the Chairman. Whenever it has been agreed that a Specimen exhibited at a Sectional Meeting of the Royal Academy of Medicine in Ireland shall be sent to the Reference Committee to report thereon as to its nature, the Exhibitor is to retain the custody of the specimen until he shall be summoned to a meeting of said Committee to be convened by its Secretary, on an early day, when he will attend and submit it for examination. (1889).

By-laws concerning "Transactions."

62. The "Transactions" shall consist of such Communications made to the Academy by or through Fellows or Members as may be deemed by the General Council suitable for publication; also, of discussions of importance or interest arising out of such Communications.

63. All Communications accepted by the Academy become the property of the Academy, but authors may also print their Communications in any publication in addition to the "Transactions." Papers shall be handed to the Secretary of the Section immediately after they have been read.

64. The "Transactions" for the year shall be presented to all Fellows of the Academy who have paid their Annual Subscriptions.

65. The "Transactions" may be purchased by Members at cost price.

66. The Publication Committee of each Section shall meet not later than the Tuesday after each meeting of the Section, for the purpose of abstracting the proceedings—the abstract to be placed in the printer's hands on same evening, and forwarded to the editors of medical journals with the least possible delay. (1888.)

67. Contributors of papers are requested to send their papers to the Academy printer early enough to allow of their being put in type before the meeting, and read in proof. (1888.)

68. That on the evening of the day of meeting of the Sectional Council, when the papers for the next meeting have been decided upon, a circular be sent to each contributor informing him :—

(1.) That he is expected to be ready or else take his place at the bottom of the list.

(2.) That he must have an abstract ready with his paper, otherwise he will be noted in the published proceedings in such form as the Publication Committee think fit.

69. The General Council is empowered to defray the expenses in whole or in part of any illustrations which it may consider advantageous to the elucidation of the papers published by the Academy.

70. An abstract (prepared by the author) of each communication made at the Academy, along with a report of the discussions thereon, shall be furnished to the editors of such medical journals as may desire to publish them, and the authors of such communications shall be empowered to publish their papers *in extenso* in any periodical or periodicals they may think fit, such communications also to appear in the "Transactions," provided the Council consider them worthy of insertion.

Expulsion of Fellow or Member.

71. Expulsion of a Fellow or Member can take place only at a General Meeting of the Academy, on the motion of the Council, if two-thirds of the Members present shall vote for the same by ballot. Of such ballot the Council must give at least fourteen days' notice in writing to every Fellow of the Academy.

New Laws.

72. New Laws, or alterations in existing Laws, can be proposed only at the Annual General Meeting. Any Fellow proposing such alteration shall give notice to the General Secretary at least ten days before the General Meeting in October.

REPORT.

The General Council have to report that during the past year the number of Fellows increased to 223, as compared with 211; that the Members have been reduced by 2 to 34, and that the Student-Associates have fallen to 2 as compared with 13. The general result has been a distinct gain, but the Council regret that the Teachers in Schools and Hospitals do not induce Students to associate themselves with the Academy early in their career. It is to be remembered, however, that since the Academy was founded three Student Societies have been formed, that they are very vigorously conducted, and that the Students probably find these a sufficient call upon their time.

The financial statement shows a balance of £17 6s. 10d. in Bank, after the payment of all expenses. In addition the Council were able to invest a sum of £125 in the $2\frac{3}{4}$ per Cent. Consolidated Stock, and the total amount so invested now stands at £491 16s. 9d.—a very satisfactory result at the close of only five years' work.

The Council suggest for consideration whether the Academy might not now try to enlarge its scope, and each year to invite a distinguished member of the profession to address the Fellows and Members on some branch of Medicine. A sum of 25 Guineas might be paid to the lecturer, who need not necessarily be a Fellow or Member of the Academy, but

who should be chosen for pre-eminence in his profession. Should the Academy approve of this suggestion, the details for carrying it out might be referred to the Council.

At the last Annual Meeting it was resolved to raise the Sub-sections of State Medicine and of Anatomy and Physiology to the position of Sections, and the further arrangements in connection with the Meetings were entrusted to the Council. It was found possible to give each of the new Sections a Meeting on two Friday evenings in the course of the Session. The Council also decided that the President and Secretary of each of those Sections should be Members of the General Council.

At a Special Meeting of the Council, on the 26th June, 1888, the following Resolution was agreed to:—

> "That this Council are of opinion that it is desirable that the Fellows of the Academy should be called together without delay, to express an opinion as to the expediency of recognising the principle that the Presidential chair should be offered alternately to a distinguished Physician and to a distinguished Surgeon, and that it should be the duty of the Council to nominate a Fellow for the Presidency at the end of every three years."

A requisition signed by ten Fellows, in conformity with Rule 52, was forwarded to the President, who thereupon sent in a letter resigning his Office, and the General Meeting was not summoned.

At a subsequent Meeting the Council declared by resolution that in their opinion the President could resign only to the Fellows at large at a General Meeting.

As the President did not call a Meeting for the purpose of tendering his resignation, and as the General Council had no

power to do so, they beg to call the attention of the Fellows to certain recommendations appended in the shape of new rules, or additions to rules, which they believe will prevent any difficulty of a like nature which might otherwise arise in future.

(Signed) S. GORDON,
Chairman.

W. THOMSON,
General Sec. and Treasurer.

TREASURERS' REPORT.

RECEIPTS.

	£	s.	d.
To Balance in Bank, 14th October, 1887 -	95	17	4
„ Subscriptions - - - - -	473	0	0
„ Dividends on £491 16s. 9d. in 2¾ per Cent. Consols - - - - - -	10	3	10
„ Messrs. Fannin & Co., part payment Illustrations - - - - - -	4	0	0
Total Receipts - -	£583	10	6

EXPENDITURE.

	£	s.	d.
General Secretary's Salary - - -	105	0	0
Printing, Stationery, Postage - - -	59	16	9
400 copies Rules - - - -	1	12	6
Postage on "Transactions" - - -	4	8	0
Printing "Transactions," Vol. V. - -	132	14	9
Illustrations - - - - -	35	14	11
Reporters - - - - -	58	4	0
Refreshments and Attendants at Meetings -	42	10	9
Sundries - - - - -	1	2	0
Invested in 2¾ per Cent. Consols - -	125	0	0
	566	3	8
Balance in Bank - - - -	17	6	10
Total - - - -	£583	10	6

We have examined the Treasurer's Statement of the Accounts of the Royal Academy of Medicine for the Session 1887-88, and we find it correct.

G. F. DUFFEY, M.D.
JOSEPH M. REDMOND, F.K.Q.C.P. } *Auditors.*

October 13th, 1888.

WM. THOMSON, *General Secretary and Treasurer.*

TRANSACTIONS

OF THE

ROYAL ACADEMY OF MEDICINE IN IRELAND.

SECTION OF MEDICINE.

ON THE ACTION OF CERTAIN DRUGS ON THE UTERO-OVARIAN SYSTEM.

By LOMBE ATTHILL, M.D.;

President of the King and Queen's College of Physicians; ex-Master, Rotunda Hospital.

[Read in the Section of Medicine, November 16, 1888.]

Much obscurity exists as to the action which medicines exercise on the utero-ovarian system, and consequently the greatest empiricism prevails in prescribing for cases in which the organs referred to are engaged. Indeed, I think it would be difficult to determine whether more injury is inflicted on women from the administration of unsuitable drugs, or from the withholding, from some prejudice or insufficient cause, such as the appearance of menstruation, of medicines in themselves proper.

The great majority of females are under the impression that no medicine of any kind should be taken during the continuance of the menstrual flow, and that to take even a mild purgative would be injurious, and many practitioners share in this unfounded prejudice; consequently the bowels are allowed to become constipated, and discomfort and even injury ensue; ordinary tonics are discontinued, and such medicines as iron and arsenic are prohibited, without any rational grounds existing for all this; while, on the

other hand, the so-called emmenagogues are prescribed in various forms of amenorrhœa at random, or as a mere routine, with the result that they in general prove useless, and sometimes positively injurious.

I have for many years been in the habit of directing my patients to continue taking the medicine I have prescribed during menstruation; and, from carefully questioning them, I am perfectly satisfied that none of the ordinary medicines, taken in moderate doses, produce any effect on the function, and it is to be doubted if the great precautions taken by many women during its continuance are, on physiological grounds, necessary.

To this statement one exception must be made; drastic purgatives, taken in large doses, do seem in many women to increase the menstrual flow, but when this does occur it is evidently due to an increased blood-supply sent to the uterus, in consequence of the stimulating action of the drug on the intestines which lie in such close contact with it.

But a much more important question arises when we come to consider the action of drugs in cases in which menstruation (*a*) does not appear at all, or very irregularly, (*b*) is in insufficient quantity, or (*c*) is unduly profuse. The medicines usually prescribed for the two first of these classes of cases are those termed "Emmenagogues," that is, "medicines which, by their stimulating action on the uterine fibre, (1) directly assist in restoring disordered menstruation, as ergot, savin, and quinine, or (2) by removing the cause of the suppression, allow the discharge to return, as iron, aloes, strychnin, &c."—("Whitla's Pharmacy," p. 128). Putting out of consideration the second class of drugs here specified, I wish to raise the question whether there are any which "directly assist the restoration of disordered menstruation by their action on the uterine fibre." In fact, are there any emmenagogues in this sense at all, and does stimulation of the uterine fibre influence the production of menstruation? I am carefully excluding at present all cases in which menstruation is too profuse or recurs too frequently, for the medicines useful in such cases should not, of course, be termed emmenagogues.

The first question is—Does ergot, savin, quinine, or even strychnin, in medicinal doses, produce any appreciable effect on the muscular fibres of the normal uterus? For in the great majority of cases in which amenorrhœa exists, the uterus is in a normal condition. To me it seems that writers and practitioners assume that these drugs have this effect, without taking any trouble to watch their action, and without sufficient grounds to justify their belief. For my part I doubt it. Absolute proof there can be none, and the theory seems to rest on the fact that ergot has, under certain circumstances, an effect on unstriped muscular tissue; but when we come to test the theory by actual practice it seems to me that the facts observed do not support it.

Quinine is administered daily in doses from 2 to 10 grains to females; but what practitioner has ever observed from its administration any symptoms indicating its having a stimulating action on the uterine fibre? I have repeatedly continued its administration during the menstrual period, and never observed it to produce any pain or discomfort, which it might be expected to do if it induced contraction of the uterine fibre, nor has it ever once among my patients exercised any influence on the amount of duration of the period. My experience of the effects of strychnin is the same. Again, I have given ergot alone, or combined with other drugs, as an emmenagogue, and have never known it to have any effect as such. I, therefore, feel satisfied that three of the most potent of the so-called emmenagogues have no appreciable effect on the unimpregnated uterus in its normal condition—normal, I mean, so far as its muscular structure is concerned. And I am forced to the conclusion either that these drugs do not, under such circumstance, produce any contraction of the uterine muscular fibre, or if they do, then that stimulation and contraction of the fibre have no effect in the production of menstruation.

Of the action of savin I know nothing. I have always looked on it as a dangerous drug, and one that is not wise to administer; for to produce any effect on the uterus it must be given in large, and therefore dangerous doses; further, that any effect it may produce on the uterus is due to irritation propagated from the intestines, just

as large doses of aloes sometimes increase the menstrual flow. Both medicines may increase, but they will not induce menstruation.

In the preceding observations I have referred to cases in which treatment has been directed with the view of inducing menstruation by mere stimulation of the uterus—a method which I consider irrational, and nearly always useless. But I shall most likely be met with the remark that the administration of drugs such as I have referred to, is sometimes followed by the occurrence of menstruation; doubtless this is so, but the recurrence of the flow by no means proves that it was the agent administered which produced this effect. Remember, I am at present speaking only of those cases in which there is no constitutional disease to be treated, such as anæmia, the cure of which is very likely to be followed by the return of menstruation, but of women, apparently in other respects healthy, in whom menstruation has been suppressed. In the great majority of these the suppression is due to some local cause, such as congestion, the result of a chill, and in such, nature is pretty sure, at least in most cases, to re-establish the function without the aid of medicines; and as the so-called emmenagogues are usually administered shortly before a period should occur, the drug may easily gain undeserved credit. Not that I am disposed to underrate the value of medicines. I only dispute the *modus operandi* of some, and I express my opinion that the drugs of the class I have specified are not entitled to be termed emmenagogues. I have repeatedly seen menstruation follow on the administration of active purgatives, of which aloes is the one most commonly used, though in plethoric women salines, especially the common Epsom salts, is even better; but then I believe that these act by unloading the venous system, and not as uterine stimulants, which latter were absolutely contra-indicated.

Here I may remark, though foreign to the actual subject under consideration, that amenorrhœa is not very infrequently met with in women whose general health is excellent. In them it evidently depends on a local cause which it may not be possible to trace—probably on some obscure affection of the nervous system—and in such patients treatment proves useless, and should not be persevered

with. One very remarkable case was that of a young lady, aged twenty-two. She came under my care early in 1884. Being the daughter of an old friend who had left his family in very poor circumstances, I took great interest in her, and I was enabled to keep her under observation for as long as I chose. I tried every medicine possible, including phosphorus and strychnin, without benefit. Then I had recourse to electricity, passing one pole of the battery into the cavity of the uterus; this caused pain, but produced no other effect. After several months I discontinued treatment. I saw the lady not long since; menstruation has not reappeared, but she is in the most robust health. I believe, as a general rule, that so long as the health continues good, and that no unpleasant symptoms, such as headache, flushing, &c., are present, it is unnecessary for the patient to undergo any special treatment with the view of re-establishing the function, and certainly that the so-called emmenagogues should not be administered. But the public, and specially mothers, are very reluctant to follow this advice, believing, as they erroneously do, that the absence of the flow is the cause of illness.

1 do not propose to discuss the action of such drugs as iron, arsenic, strychnin, &c., which are classed as emmenagogues by writers, because "by removing the cause of the suppression they allow the discharge to return," but content myself with saying that it is a misnomer to call them emmenagogues. As well might we call any drug prescribed in a case of incipient phthisis, in which amenorrhœa existed, and which, on the patient's recovery, disappeared, an "emmenagogue," and I trust the word will in time disappear from our works on Materia Medica.

But one drug requires a passing remark—I allude to the permanganate of potassium, which, on the ground that it yields oxygen to the blood, has recently been much lauded as an emmenagogue. I have prescribed it extensively, and always without result, and my belief is that the theoretic value attributed to it as a therapeutic agent is groundless—at least when administered in the doses usually ordered.

I now turn to the consideration of the action of drugs in cases in

which menstruation is too profuse, or in which actual uterine hæmorrhage occurs, putting aside those cases which depend for their origin on abortion, pregnancy, or parturition. Excluding these we find that menorrhagia and metrorrhagia are generally met in connection with—1st, ovarian congestion; 2nd, the presence of uterine tumour; 3rd, malignant disease of the uterus; or 4th, imperfect involution of the uterus, which generally co-exists with a hypertrophical condition of the intra-uterine mucous membrane. This last form I shall dispose of in a few words. It nearly always demands special local treatment, carried out with the view of restoring the mucous membrane to a healthy condition, and unless this be done medicines seldom prove of much value.

The form of ovarian congestion to which I now allude must not be confounded with that met with in cases of chronic disease of the ovaries and tubes, and in which, though menstruation may be painful, it is rarely profuse. But it is that form most frequently seen in young females, and occasionally in women who are approaching the climacteric period, and in whom menstruation is preceded by pain referred to one or both ovaries, and sometimes with well-marked mammary sympathies. The discomfort some women suffer before each menstrual period is very great, but it is generally relieved by the appearance of the flow, which is often very profuse. Fortunately this can nearly always be cut short, or entirely prevented, by the timely exhibition of full doses of the bromide of sodium, ammonium, or potassium. It is necessary, however, to commence the administration of these drugs before the recurrence of the symptoms is expected, which, in the majority of cases, is five or six days prior to the appearance of the flow; and also to see that the dose of the bromide be not less than half a drachm, taken three times a day. I do not think that the addition of ergot or any other drug increases the efficacy of the bromide, but if the woman be plethoric a dose of some brisk saline purgative often materially assists in relieving the patient and in diminishing the loss, which is sometimes excessive.

Of uterine tumours, whose presence may induce hæmorrhage, there are two kinds—the pedunculated polypus and the fibroid.

No drug seems to have much effect in checking the loss due to the presence of a pedunculated polypus, but in the case of the true uterine fibroid it is different, and the more perfectly the tumour is surrounded by the muscular tissue of the uterine wall, the more marked will be the effect of treatment.

The medicines known as astringents may at once be discarded as useless in cases of uterine hæmorrhage. I do not believe that tannin, tannic or gallic acid, or any other astringent, has any effect whatever in these cases—indeed their administration is worse than useless. I believe it to be injurious, and the mineral acids seem to me to be of no greater value. Full doses of the tincture of the perchloride of iron have, in my hands, sometimes been of great use in checking the loss, but this was always in anæmic women, and its beneficial effects were probably due to the iron it contains—but at best it is a very unreliable agent. Ergot is the only drug on which I place any reliance, and there is only one preparation of it which is trustworthy.

Some years ago I made a very extended trial of the various preparations of ergot in common use, both by the mouth and hypodermically, and came to the conclusion that none of them contained all the active principles of the drug. Consequently, for many years I have been in the habit of prescribing twenty or thirty minims of the liquid extract B. P. in half an ounce of the infusion of ergot, with, in general, the addition of a small quantity of the liquor strychninæ, with fair results; but I have found the American extract, known as "Squibb's Ergot," to be so superior to that of the British Pharmacopœia that I now rely almost entirely on it.

Here is one remarkable example of its good effects:—In October, 1887, I was consulted by a lady, aged about thirty-eight years, and who was the subject of a very large fibroid, which extended from above the umbilicus to within an inch and a half of the vulvæ. Her sufferings were intense, and she was never free from pain except for a few days at the conclusion of each menstrual period. The interval between each period was only about seven or eight days, the hæmorrhage continuing more or less for twenty or twenty-one, and very profusely for the first week. She had consulted a number of

medical men, one of whom had suggested the removal of the tumour, and she came to me mainly with the view of having my opinion on this point, saying she had made up her mind to submit to the operation if I advised it. On the 2nd of December I examined her under chloroform, and then discovered that there were apparently two tumours—one quite movable, the other nearly fixed. The size and position of the fixed tumour induced me to come to the conclusion that unless life was absolutely in danger, an operation would involve too great a risk to be undertaken, and this I told her friends. On the 21st of December, having in the interval put her on iron and arsenic, I prescribed Squibb's ergot—thirty drops to be taken three times a day, commencing before the change was due, and to be continued till the loss had quite ceased; iron and arsenic to be taken in the interval, the ergot to be recommenced before each period. On the 15th of February she reported herself as being decidedly better, and I saw no more of her till the 25th July last. She was then quite a different person—she suffered very little pain, could walk a considerable distance, had twenty-one days' interval between the periods—which, though still very heavy, lasted but seven days—and were it not that she suffered from discomfort after food and slept badly, would have had little to complain of. As I have not seen this lady since July I think I may assume that she has continued to improve.

A remarkable feature in this case was the relief from pain experienced by the patient, and which was probably due to the tonic contraction of the muscular fibres of the uterus, whereby the blood-supply was diminished and the hæmorrhage lessened.

Ergot is a drug most uncertain in its action in these cases and in the effects it produces. In some it causes pain, and when it does it always, I think, lessens uterine hæmorrhage, the pain being evidently due to clonic contraction of the muscular fibres. But sometimes the same dose of the same preparation which caused pain previously does not do so on another occasion, though as far as we can judge no change has taken place in the patient's condition. I am inclined to the opinion that ergot will not induce clonic contraction of the uterine fibres, unless something acting as a foreign body be present in it. I say "acting" as a foreign

body, because the mere presence of such in the uterus is not sufficient. Pedunculated polypi are commonly enough met with in the uterus, but their expulsion by painful uterine action is quite rare, and it is most likely that the seat of the tumour is the main element of its tendency to excite uterine action. The portion of the uterus between the entrance of the Fallopian tubes is the sensitive portion of the organ, and it is, in my opinion, necessary for a tumour to be situated there for it to act as a foreign body.

I may here point out that it is very doubtful if ergot ever originates clonic contractions of the uterus during pregnancy, unless the organ is prepared from some pre-existing cause to expel its contents. When engaged formerly in midwifery practice I was in the habit of frequently prescribing ergot as a preventive to *post-partem* hæmorrhage, commencing its administration a week or ten days before the expected advent of labour, and never once had I reason to suppose that it hastened that event—on the contrary, in several the period of utero-gestation seemed to be lengthened. In like manner, in cases of a threatened abortion, I have seen the hæmorrhage checked, and pregnancy proceed normally under the administration of ergot; it seemed, indeed, to act as a uterine tonic, if such an expression be admissible. In others, and perhaps the majority, it seemed to produce no effect at all; in a few it induced clonic spasms, but in these there was always reason to think that the ovum was already blighted. In cases of uterine fibroids ergot will, in general, be found to act most beneficially in lessening hæmorrhage when the tumour is embedded in the muscular tissue, and as thinning of the wall takes place, and as the tumour consequently comes in closer contact with the uterine mucous membrane, the result of its administration will be less satisfactory; but in all cases much will depend on the preparation used and its freshness.

With respect to the action of medicines in arresting hæmorrhage where malignant disease of the uterus exists, I have little to say—none can be relied on. I have sometimes thought that the exhibition of Chian-turpentine, as recommended by Dr. Clay, did good in this respect, but further experience has lessened my faith in it. Tincture of the perchloride of iron, in full doses, also sometimes is useful, but at best any good done is but transitory.

ACCIDENTAL RASHES IN TYPHOID FEVER.

By JOHN WILLIAM MOORE, B.A., M.D., M.Ch., Univ. Dubl.;
Fellow and Registrar of the King and Queen's College of Physicians;
Physician to the Meath Hospital.

[Read in the Section of Medicine, November 16, 1888.]

In the remarks which follow, it is not my intention to allude to the essential rose-spot rash of typhoid fever; or to the more common epiphenomena of the disease connected with the skin, such as *taches bleuâtres*, purpura-spots, vibices, and sudamina or sweat-vesicles;[a] or, lastly, to the co-existence with typhoid fever of other specific diseases showing characteristic eruptions, such as scarlatina, measles, variola, and—above all—typhus. I desire rather to draw attention to certain other accidental or adventitious appearances of the skin, which are of a somewhat rare occurrence, and, from a diagnostic point of view, of considerable importance. These are—(1) Simple Hyperæmia; (2) Miliary Eruptions; (3) Erythematous Rashes; and (4) Urticaria.

Case I.—*Simple Hyperæmia* or *Erythema Fugax* (Radcliffe Crocker).—So recently as November 9, 1888, I visited at 9 30 a.m. a gentleman, aged twenty-two, who had been for the previous five weeks passing through a mild attack of typhoid fever, in which the temperature had probably never exceeded 104° F. I say "probably" because the record of evening temperatures is incomplete. A marked symptom in the case was repeated and severe epistaxis. Rose-spots had appeared in successive crops, and there was decided enlargement of the spleen. The bowels had been regular throughout—no interference with their action

[a] "La miliaire pellucide, improprement appelée *sudamina*, qui apparaît ordinairement du onzième au vingtième jour, quelquefois plus tard, est constituée par de petites bulles, arrondies ou oblongues, ressemblant alors à des larmes, remplies d'un liquide transparent." Trousseau. Clinique Médicale de l'Hôtel Dieu. 1865. Tome I., p. 234.

being at any time required. A week of apyrexia having passed by, the convalescent was allowed a small portion of the breast of a chicken on Wednesday, November 7. Next morning the tongue was furred; the pulse rose to 96 and the temperature to 99·4°. In the forenoon, the patient felt chilly, and on attempting to get up about 3 p.m., he was seized with a violent rigor. He said he did not *feel* cold, but nevertheless he *shook* all over. I saw him at 6 p.m., and found him with a pulse of 136 and a temperature of 105·5°—a perspiration was just setting in. Next morning the pulse had fallen to 116 and the temperature to 99·8°. A profuse diaphoresis had occurred in the night. The skin was everywhere suffused—the rosy blush being deepest near the armpits and the flexure aspects of the joints. On drawing the finger across the surface no so-called *tache scarlatinale* could be seen. There were a few sudamina. The throat was not sore, although the mucous membrane of the fauces was injected. Quiet sleep was enjoyed during the day, and by 6 30 p.m. the pulse had fallen to 108, and the temperature to 98·8°, with a moderate perspiration. The bowels acted naturally and freely about 3 p.m. On Monday, November 12, at 10 a.m. the patient was apparently convalescent—tongue clean, pulse 68, and temperature 97·2°; abdomen soft, bowels regular.

There can be no question that this was a case of pyrexia and simple hyperæmia, or intense determination to the skin, of an eliminative kind, resulting perhaps from the inhibitory effect of an intestinal irritation on the vaso-motor system.

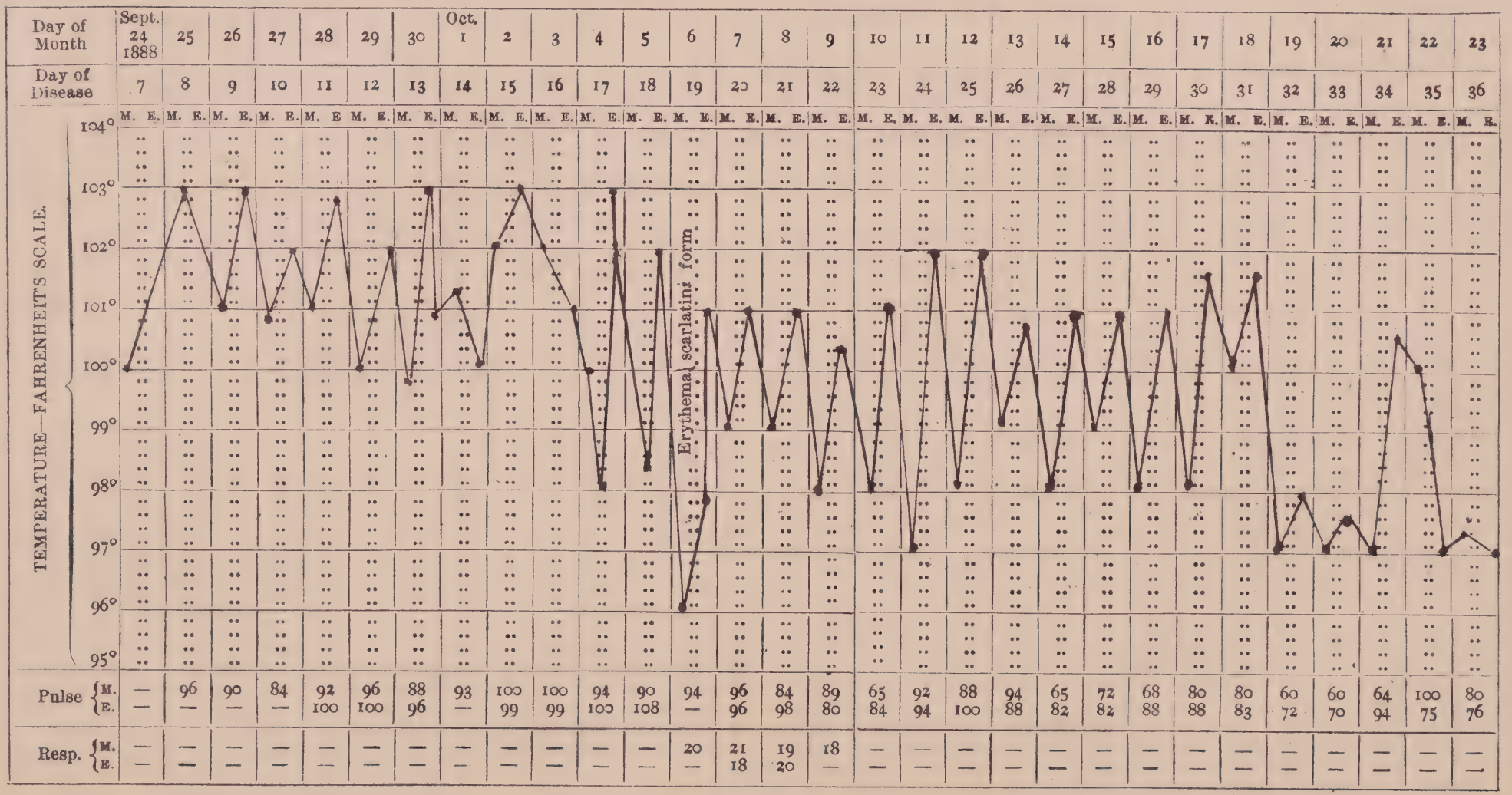
Case II.—*Miliary Eruptions.*—On Thursday, September 27, 1888, Mr. George M. Foy, F.R.C.S., asked me to see with him in consultation a gentleman, twenty-four years of age, residing at Rathmines, who was then about ten days ill of typhoid fever. The attack had commenced with severe headache and constipation. There was some splenic enlargement, and a few rose-spots were scattered over the arms and buttocks. The pulse (at 1 p.m.) was 104, respirations 26, and temperature 102·5°. It was impossible to ascertain whether rose-spots were or were not present on the trunk, as it was everywhere covered with a dense miliary rash, each tiny vesicle being filled with a lactescent fluid and surrounded with a zone of hyperæmia, resembling a typhus macula or a measles papule. At first sight, on looking at the back, one was inclined to

believe that the case was a severe typhus fever, with a copious and almost petechial rash. Two facts belied any such view. First, this crop of miliary vesicles had appeared within the previous twenty-four hours; and secondly, the rash was strictly limited to the trunk. I have occasionally seen a precisely similar appearance in acute rheumatic arthritis, or rheumatic fever.

Case III.—*Erythema Simplex*, vel *Scarlatinale.*—On Sunday, October 7, 1888, I was asked by Dr. J. F. Pollock, F.K.Q.C.P., to see with him a young gentleman, aged nineteen, at Blackrock, Co. Dublin, at that time on the twentieth day of typhoid fever. The reason for calling me in was the appearance some hours previously of a wide-spread rash, which exactly resembled that of scarlatina. It consisted of minute punctiform bright-red papules, with a general or confluent efflorescence on the trunk and near the large joints, particularly on their flexure aspect. The question I was called upon to solve was whether the rash was really that of scarlatina or not. It was of the gravest importance that no mistake should be made, as the patient happened to be on a visit in a house full of young children, none of whom had, I believe, suffered from scarlatina. Early in September the gentleman complained of diarrhœa, for which he was treated by Dr. Pollock, and apparently recovered in a few days. He went to the country, but soon began to ail. On Monday, September 24, he again consulted Dr. Pollock, who found his temperature high and sent him to bed. It was soon evident that he was about a week ill of typhoid fever. The disease ran a tolerably normal course until the 6th of October (19th day), when an alarming "dip" in the temperature occurred. At 10 30 a.m. a minimum of 96° F. was recorded. In the afternoon a recovery took place, and at night a reading of 100·9° was noted. At this time a scarlatinal rash was rapidly spreading over the chest and back, and next day it was distributed as I have already described. The tongue was stripped of its epithelium, glazed, with enlarged papillæ—a condition not unusual towards the close of the third week of typhoid fever. There was no undue redness, swelling, or tenderness of the fauces; the cervical glands were not swollen. Was scarlatina present? I answered unhesitatingly "No," and for these reasons. There was no history of exposure to infection within the previous week. There had been no rigor, or nausea, or vomiting, or diarrhœa on the preceding day. There was no sore throat or glandular swelling. The pulse did not rise above 108. The tem-

CHART OF TEMPERATURE, in the Case of Mr. A. B., age 19, under the care of DR. J. F. POLLOCK.

Disease, Typhoid Fever; *Result*, Recovery.



perature ranges were not in any way characteristic of scarlatina. The condition of the tongue was quite as likely to occur in typhoid fever as in the even more dreaded disease which was suspected to be present. Lastly, rashes of the kind are not very uncommon in acute fevers, although indeed it is in the earlier stages that such epiphenomena are usually seen.

Dr. Pollock has very kindly given me a copy of the clinical chart in his patient's case, and has informed me that considerable desquamation followed the disappearance of the rash, which, however, he concurred with me in regarding as not that of scarlatina. The clinical chart is appended, and affords a good example of typhoid fever of medium severity, running its course in about five weeks. Albuminuria was not observed.

Case IV.—*Urticaria* (Germ. *Nesselsucht*).—I am indebted to Dr. Walter G. Smith for a brief note of a case of typhoid fever in which this complication occurred. He was recently called to the country in consultation to see a man, aged forty-five years, who was at the time of his visit in the fifth week of the fever. The immediate reason for Dr. Smith's visit was a sudden and extensive outbreak of urticaria or nettle-rash. The wheals were very distinct, and there was troublesome itching. In a day or two the eruption faded and the irritation of the skin subsided.

Now, of all these adventitious appearances connected with the skin in typhoid fever, the erythematous rashes are the most important, because they may so closely simulate scarlatina. The miliary eruption, if coupled with the rheumatoid pains which are not infrequently present in typhoid, might cause a wrong diagnosis of rheumatic fever to be made; while nettle rash, if associated with catarrh, might be mistaken for measles. But, serious as we should consider these errors of diagnosis to be, a much more disastrous mistake would be the confounding of erythema with scarlatina, or, conversely, the overlooking of scarlatina in the belief that the rash was simply erythema. The third case I have detailed in this paper is an instance in point. In giving my opinion, I was fully aware of the grave responsibility incurred, and of the issues which were at stake; and yet with confidence I decided against the diagnosis of

scarlatina, being convinced that the presence of a scarlatiniform rash was not by itself sufficient to warrant or to justify a diagnosis of scarlatina.

What, then, was the eruption? There can be little doubt that it was the *Erythema punctatum* of Dr. M'Call Anderson,[a] the *Erythema scarlatiniforme* of Hardy, the *Roséole scarlatiniforme* of Basin, the *Symptomatic Erythema* of Dr. James Nevins Hyde,[b] the *Erythema scarlatiniforme* of Dr. H. Radcliffe Crocker,[c] the *Diffuse Erythema* of Liebermeister,[d] the *Scarlet Rash* of Charles Murchison.[e]

Writing of this rash Dr. Hyde observes:—"Several of the severer constitutional maladies betray their morbid influence upon the central nervous system by a speedy efflorescence of this character. A lurid erythema of the axillary or inguinal region may precede by several days the eruption of confluent variola. Cholera, cerebro-spinal meningitis, enteric and other fevers are thus at times accompanied, preceded, or followed by rashes. A study of these is of the utmost importance to the diagnostician. Children who are really susceptible to the disease are often supposed to possess an immunity from scarlatina, as the symptomatic erythema they previously displayed was misconstrued."

Liebermeister says:—"In some cases [of typhoid fever] that I have seen, a *diffuse erythema* has appeared, in the course of the first week, most commonly on the neck and forearm, but sometimes on other parts. Occasionally this has been so diffuse that it caused doubt for a little while, especially when the patient had sore throat, whether one might not be dealing with a case of scarlet fever. This eruption afterwards disappeared, and the disease ran its ordinary course. So far as I can remember, these were all severe cases."

[a] A Treatise on Diseases of the Skin. London: Charles Griffin & Co. 1887. Page 89.

[b] A Practical Treatise on Diseases of the Skin. Second Edition. London: J. & A. Churchill. 1888. Page 154.

[c] Diseases of the Skin. London: H. K. Lewis. 1888. Page 58.

[d] Von Ziemssen's Cyclopædia of the Practice of Medicine. London: Sampson Low, Marston, Low, and Searle. 1875. Vol. I., page 183.

[e] The Continued Fevers of Great Britain. Third Edition. London: Longmans, Green & Co. 1884. Page 516.

To very much the same effect Murchison writes:—"In many cases of enteric fever the appearance of the lenticular spots is preceded, for two or three days, by a delicate scarlet rash all over the body, disappearing on pressure. This is not peculiar to enteric fever, but occurs in other forms of pyrexia. It is best seen in patients with white delicate skins. Jenner mentions an instance where this rash co-existed with slight sore throat, and the disease was mistaken for scarlatina. Several similar cases have come under my own notice. Occasionally this hyperæmia persists throughout the fever, and in the advanced stages a red or purplish blush of the skin is sometimes observed on the dependent parts of the body."

But, not to multiply quotations, we find the fullest account of this puzzling rash in Radcliffe Crocker's work on "Diseases of the Skin," in the following words:—"Under certain circumstances a widespread or general eruption comes out, sometimes quite suddenly, punctiform, erythematous, and exactly resembling scarlet fever in most of its features; but it does not begin in any special position, and moreover it is common to find the eruption sharply defined in places, especially beside the nose, leaving a narrow tract of, by contrast, preternaturally white skin between the two hyperæmic areas. This is never the case in scarlatina. Like the other erythematous eruptions, the red skin is whitened for a moment when the finger is drawn across it. There is some constitutional disturbance, usually slight, the temperature being 100° or 101°, but soon subsiding, and the fauces somewhat red; if the general symptoms are severe, they are due to the disease in whose course the eruption appears. The subsidence of the rash, which occurs in from two to six days, is usually, but not always, followed by more or less copious desquamation, according to the intensity of the erythema."

On April 13, 1883, Dr. Thomas Whipham read a paper before the Clinical Society of London, entitled "Two Cases of Enteric Fever accompanied by an Erythematous Eruption resembling that of Scarlatina."[a] In his paper Dr. Whipham refers to an elaborate communication, by Dr. John Harley, on the question of the con-

[a] Transactions of the Clinical Society of London. Vol. XVI., page 150.

nection between scarlatina and enteric fever,[a] and concludes with the following paragraph: "It seems, therefore, difficult to avoid the conclusion that the last three of Dr. Harley's cases, together with the two which form the subject of this communication, are in reality instances of enteric fever preceded by an erythema, which is not uncommon in small-pox, and which is frequently observed in pyæmia." The most important point about Dr. Whipham's two cases is that they both proved fatal, and that an opportunity was afforded in each instance of verifying the diagnosis of typhoid fever by a *post-mortem* examination.

According to an abstract from the pen of Dr. Robert Saundby in the *London Medical Record* of December 15, 1878, MM. Raymond and Nélaton reported in *Le Progrès Médical* of October 19 of the same year, the histories of two cases of typhoid fever in the wards of M. Moutard-Martin, at the Hôpital Beaujon, which were accompanied by a very singular eruption, both in its aspect, its course, and the time of its appearance. MM. Raymond and Nélaton remark that analogous rashes have been recorded, in particular by Griesinger, who says:—"In rare cases we observe, at the same time as the roseola [*i.e.*, the rose-spot eruption], a rash *like* urticaria." To their two cases MM. Raymond and Nélaton add a third noticed by M. Moutard-Martin, in which a rash, resembling that caused sometimes by copaiba, and which he called first "febrile urticaria," but afterwards "papular erythema," supervened on the sixth day of typhoid fever. The rash gradually lost its bright colour, and slowly disappeared. The hands and feet desquamated in large patches; the rest of the body showed only branny desquamation. As to the cases observed by MM. Raymond and Nélaton, they believe that they were not scarlatina, for the following reasons:—1. The form of the eruption, and its predominance in parts unusual in that disease. 2. The absence of prodromata, and of the concomitant scarlatinal symptoms, angina, state of tongue, &c. 3. The course followed by the desquamation. On the hands there were no large flakes. One of the patients showed the rash on the fifteenth day of his fever; the other

[a] Medico-Chirurgical Transactions. Vol. LV., page 103.

showed it on the eleventh or twelfth day. Both cases were very severe, and were of an ataxo-adynamic type.

In the Proceedings of the "Société Médicale des Hôpitaux de Paris" for 1873, M. Siredey reports a case in which "towards the fifteenth day of a typhoid fever a scarlatiniform erythema appeared, which, commencing in the right cheek, spread rapidly over the face and neck, and by the following day occupied the whole surface of the body. Almost immediately, in the groins, the axilla, and on the back, the epidermis was raised by a serous exudation." M. Guyot, consulted by M. Siredey, agreed with him in rejecting the notion of its being scarlatina; nevertheless, desquamation occurred in large flakes.

It is not easy to arrive at a satisfactory conclusion as to the ætiology of this accidental erythema in typhoid fever. Bearing in mind the fact that it most commonly occurs early in the disease—that is, when a determination of blood to the skin may be presumed to have followed the initial rigors of the fever—we may, with some show of reason, attribute the development of the exanthem to an inhibition of the vaso-motor centre—the correlative of the stimulation of that centre by which the precedent rigors are explained—causing relaxation and overfilling of the arterioles of the skin, the impure blood further producing a passing dermatitis in the form of an erythema.

When the rash happens in the second or third week, we should remember that we are approaching the sweating stage of typhoid fever, and that then there is again a determination of impure blood to the skin. Or we may regard this later appearance of the rash as of septicæmic origin, agreeing in time with the period "spiking temperature," which is itself so suggestive of a septicæmic tendency. As Dr. Radcliffe Crocker says:[a] "It [erythema scarlatiniforme] is seen occasionally in septicæmic conditions, as after surgical operations, but not so frequently since antiseptic precautions have been generally adopted." And this may throw some light on M. Trélat's observations on "Scarlatina following Surgical Operations," which appeared in *Le Progrès Médical*, September 14,

[a] *Loc. cit.* Page 58.

1878,[a] as well as on the supposed origin of scarlatina from the decomposition of slaughter-house refuse, which was suggested, many years ago, by Dr. Alfred Carpenter, of Croydon.[b]

In the *British Medical Journal* for December 7, 1878, there is a very interesting note on "Septicæmic Eruptions." In 1868, M. Verneuil proved the existence of several different cutaneous eruptions as taking place during purulent infection. About the same time, Dr. Braidwood was giving special attention to the study of one of those peculiar forms of pyæmic eruptions which bear a very close resemblance to the erythema observed in scarlatina. M. Guéniot tried to prove, in accordance with the well-known fact that women who have been recently confined are often subject to cutaneous eruptions, that scarlatiniform erythemata had been observed, the so-called puerperal *scarlatinoïde.* In addition to the above-mentioned two classes of eruptions, M. Claudien Aulnas adds a third, which has also been observed in the course of purulent internal diseases. I submit that here we have sufficient evidence of the septicæmic or pyæmic origin of erythema scarlatiniforme, whether in typhoid fever or in other zymotic diseases or blood-poisonings.

In the four cases detailed in this paper, there was no evidence to connect the skin-rashes with the administration of any drugs. The patient, who was the subject of Case I., had not been taking any medicine whatever for a week or ten days before the efflorescence appeared. In Case II., only some small doses of morphin had been given. In Case III., quinine in 2-grain doses, thrice daily, had been given at the beginning of the attack, but had been discontinued because the medicine seemed to disagree. I have not heard that the patient whom Dr. W. G. Smith saw was at the time taking any medicine at all.

It is necessary to make this statement, remembering, with Dr. Radcliffe Crocker,[c] that "a precisely similar rash occurs after

[a] See Lond. Med. Record, Oct. 15, 1878. Page 417.

[b] See the Lancet for January 28, 1871, page 110; and Manual of Public Health for Ireland, 1875, page 169.

[c] *Loc. cit.* Page 58.

certain drugs, especially copaiba, quinine, belladonna, and salicylic acid, &c. In these cases the rash is probably due to irritation of the alimentary canal acting reflexly on the vaso-motor centre."

To sum up:—

1. Not infrequently, in the course of typhoid fever, an adventitious eruption occurs, either miliary, urticarious, or erythematous.

2. When this happens, a wrong diagnosis of typhus, measles, or scarlatina respectively may be made, if account is not taken of the absence of the other objective and subjective symptoms of these diseases.

3. The erythematous rash is the most puzzling of all; but the prodromata of scarlet fever are absent, nor is the typical course of that disease observed.

4. This erythema scarlatiniforme is most likely to show itself at the end of the first, or in the third, week of typhoid fever.

5. In the former case, it probably depends on a reactive inhibition of the vaso-motor system of nerves; in the latter, on septicæmia, or secondary blood-poisoning; or both these causes may be present together.

6. The cases in which this rash appears are often severe; but its development is important rather from a diagnostic than from a prognostic kind of view.

7. Hence, no special line of treatment is required beyond that already employed for the safe conduct of the patient through the fever.

ON A CASE OF CHROMIDROSIS.

By ARTHUR WYNNE FOOT, M.D., Univ. Dubl.;
Senior Physician to the Meath Hospital.

[Read in the Section of Medicine, December 14, 1888.]

After the exhibition of several characteristic coloured drawings of patients with chromidrosis, Dr. Foot remarked:—

In tracing the history of the disease, from the first case recorded by Dr. Yonge, of Plymouth, in 1709, up to the present year (1888), I find that there are forty-six admittedly genuine cases on record—cases which have passed repeatedly through the crucible of criticism, and to which no exception can be taken. Of these forty-six cases six have occurred in men, and forty in women—for the most part young and unmarried. That so small a number as forty-six cases should have been reported in the 180 years since Dr. Yonge first drew attention to it, I consider to be a proof of the rarity of its occurrence. That it is a curious and striking affection is shown by the amount of incredulity which has often been exhibited in regard to it. As recently as the year 1861 the Medical Society of the Hospitals of Paris gave an open verdict on the subject, notwithstanding the strong pleading of M. Hardy in its favour; and in the same year the Imperial Academy of Medicine declined to allow de Méricourt's essay on chromidrosis to be published in their memoirs, on the ground that the authenticity of the cases was doubtful, although the Committee which they had appointed to report on it recommended it merely for publication, without asking for the expression of any opinion on its merits. On April 4th, 1882, a Committee of the Clinical Society of London reported "that chromidrosis has an undoubted existence as a genuine affection."

I am not inclined to follow Féréol in substituting the name

chromocrinia for chromidrosis; for if the views of Meissner and Unna are correct, that the coil of the sweat gland secretes fat and the end of the duct sweat, disorder of the coil glands would account for all the phenomena.

I am an advocate of the theory that the pigment is due to the oxidation of indican eliminated by the cutaneous glands: its preference for the face may be explained by the free exposure of that area to the great oxidising influences of air, light, and heat. Some ferment also might operate in effecting the conversion of soluble colourless indican into indigo-blue or brown. The possibility of a bacterial origin is not to be overlooked, as the source of the colour in "blue milk" is often the *B. cyanogenus* and in red sweating *Bacterium prodigiosum*. The indican, I consider, is derived from the absorption from the intestinal canal of small quantities of indol. The frequency of constipation in chromidrosis, the sudden disappearance of the colour after purgation, its diminution after regulation of the bowels, and the general aversion to animal food these cases exhibit, are facts which make in the direction of this view. The proportion of indican in the urine increases, *pari passu*, with the amount of exudation on the face, and *vice versâ*. Hoffmann has detected indigo in the bluish-black exudation, and after reducing the pigment to indigo-white it was seen to regain its colour by exposure to the air.

A MIXED CASE OF TYPHUS AND TYPHOID FEVERS; DEATH AND POST MORTEM EXAMINATION.

By C. J. NIXON, M.D.;
Senior Physician, Mater Misericordiæ Hospital, &c.

[Read in the Section of Medicine, December 14th, 1888.]

The case which I have the honour of bringing before the Academy to-night will prove, I believe, of interest to those who are familiar with the difficulty which occasionally arises in determining during life the form of fever which exists. Whilst in the great majority of instances, the origin, mode of invasion, symptoms and course of typhus and typhoid fevers render their distinction, one from the other, a matter of ease and certainty, sometimes the symptoms of the two affections are so imperfectly marked, so complex, so disposed to partake of characters peculiar to both fevers, that the differential diagnosis may be, with good reason, indefinitely postponed. We meet, moreover, with cases where the broad distinctive signs of both typhus and typhoid coexist, and as an illustration of this, the following case is recorded:—

Case.—J. C., aged twenty-two, a grocer's assistant, was admitted into the Mater Misericordiæ Hospital on the 16th of November last. He had been seen, previously to admission, by Dr. M'Evoy, of Blackrock, who sent the patient to me as suffering from typhoid fever. He had had diarrhœa of an urgent character, and had suffered from epistaxis. When seen on the day of his admission the patient presented most of the ordinary symptoms of typhoid fever: diarrhœa, epistaxis, a well-marked crop of raised rose-coloured spots visible over the front of the chest and abdomen, sparsely scattered, and not marked upon the posterior aspect of trunk; enlargement of the spleen determinable by palpation; moderate distension of

abdomen, but no marked tenderness or gurgling over ileo-cæcal region. The physiognomy was that of enteric fever ; there was a clear eye, with absence of conjunctival congestion, and the patient seemed bright and cheerful. The tongue was more thickly and uniformly coated than is usually seen in typhoid. There was some headache of a frontal character. The pulse was 100, the temperature 102·6° F., and the respirations 24 in the minute. The diagnosis made of the nature of the case was typhoid fever.

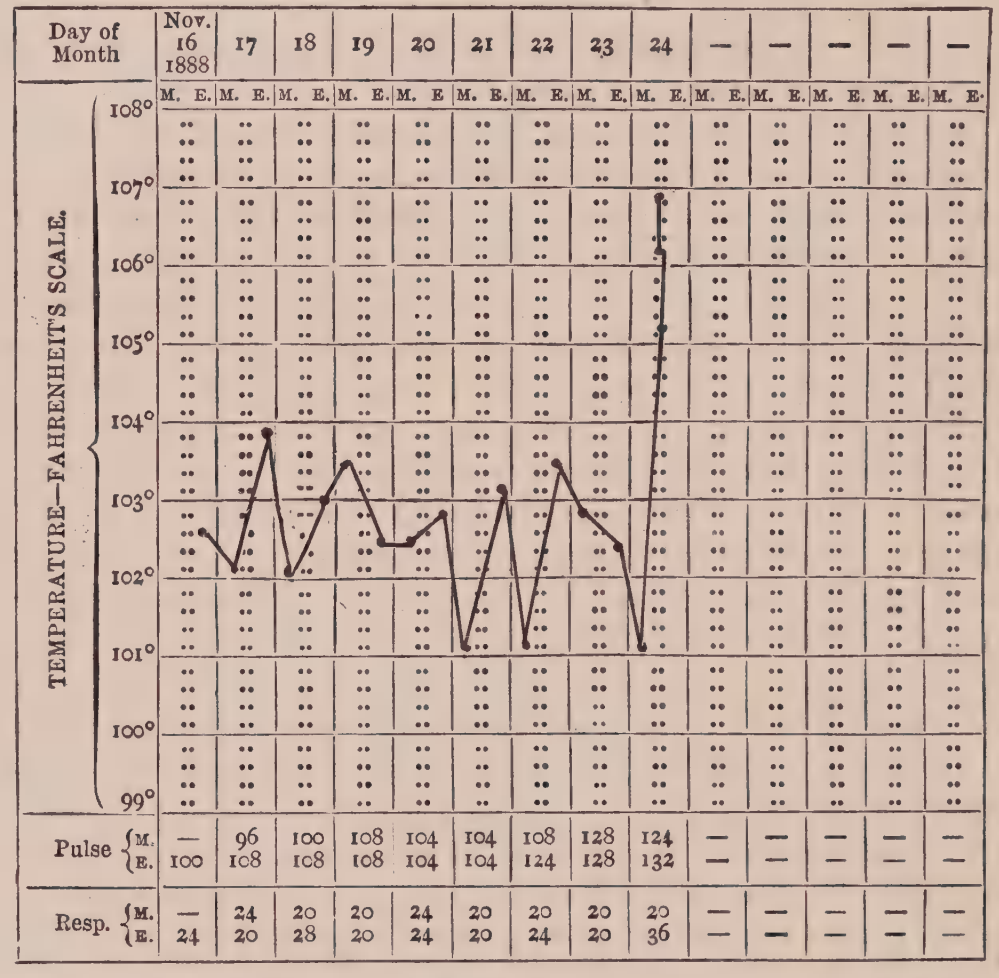
I did not see the patient on the following day, but on the day afterwards when I entered the ward the Sister in charge said to me, "The patient C. must have typhus, as he is covered with a typhus rash." I found, as the Sister stated, that the patient was profusely maculated. The spots were so thickly clustered upon the face as to resemble a case of measles, and they appeared profusely upon the trunk and parts of the extremities, especially upon the back of the wrists. The rose spots noted two days before, were still to be seen, raised and contrasting by their colour with the darker spots which surrounded them. The condition and aspect of the patient had changed considerably. He was dull and stupid ; the conjunctivæ were suffused and the tongue had become dry and brown along the centre. Diarrhœa was still persistent, and the stools were perfectly fluid in consistence, but of a dark brown colour. There was a tendency to delirium of a low muttering character, but only existing at night. I confess I was somewhat puzzled as to the exact nature of the case. However, what I stated after my examination on the 18th of Nov., the third day of the patient's stay in hospital, was that the case would be considered by some physicians as a mixed case of typhoid and typhus fevers, but that I did not believe in mixed cases, because in all the instances where that diagnosis had been made, in my own experience, the course of the disease showed that one form of fever alone existed, and I was therefore inclined to regard the expression "a mixed case" as merely a *façon de parler* which helped the physician to cover what might be a legitimate incompetency on his part to make an accurate differential diagnosis. Entertaining this view and having regard to the prominent symptoms present, I looked upon the case as one purely of typhus fever, and I considered I had been in error in the diagnosis previously made of typhoid. The course the case ran was markedly that of typhus :—Increasing stupor, typhomania, maculæ changing into petechiæ, progressive dryness and brownness of the tongue, sordes upon the teeth, and signs of failure of circulation.

The chart exhibited shows the course of the disease :—

CHART OF TEMPERATURE, in the Case of J. C., age 22, under the care of DR. C. J. NIXON.

Disease, Typhoid and Typhus Fevers; *Result*, Death.

Died at 10 o'clock.



On the 24th of November, when discussing with the hospital class conditions of the tongue which are characteristic of certain forms of disease, I took them to the fever wing to demonstrate a condition of tongue that I believe is met with more frequently in typhus than in any other affection—viz., the dry brown tongue which in bad cases, towards the end of the fever, becomes retracted into a convex mass, covered with a shiny blackish cracked crust, and incapable of being protruded by the patient. But the characteristic marks of a typhus tongue no longer existed in C. It had cleaned, the rash had almost vanished, the patient appeared much brighter, and without any duskiness of the face, or conjunctival suffusion. The temperature had fallen nearly two degrees from the previous

day, reaching 101·2° F., so that the appearances noted indicated that crisis was taking place, and apparently under favourable conditions. There was no complication of any serious kind, save the persistence of diarrhœa, to the extent of three to four stools in the twenty-four hours. The case, to all appearances, was progressing satisfactorily, and there were reasonable grounds for a favourable prognosis. At 6 o'clock on the evening of this day, the ninth of the patient's stay in hospital, the twelfth, as far as we can ascertain, of the existence of the fever, the Sister in charge of the ward gave the patient his usual evening cup of milk and brandy, and he then seemed to her to be progressing satisfactorily. A quarter of an hour afterwards, in passing through the ward, she heard him groan, and on going over to the bed she found that some momentous change had taken place. He complained of a chill, and was sweating profusely. Fifteen minutes before there was no sign of sweating, but he was now as if he had been lifted out of a bath, large beads of perspiration rolling from the forehead and face. The house physician at once saw the patient, found the temperature 105° F., the pulse 102, and the respirations 36. The feet were inclined to be cold, and the pulse was weak and thready. The usual restorative treatment was adopted, and the patient, who was quite collected, expressed himself as feeling better and stronger, but the temperature steadily rose until it reached 106° F. at 8 o'clock, and 106·8° F. at 9 o'clock, when he gradually sank.

The *post-mortem* examination was carefully made sixteen hours after death by Dr. M'Weeney, Pathologist to the hospital. All the viscera were examined except the brain and spinal cord. The following appearances were noted:—

Rigor mortis well marked; dark purple discoloration of the posterior part of the trunk, and patchy discolorations of the extremities.

Muscles of a dark red colour; blood black and fluid.

Heart soft and flabby; cavities nearly empty, containing merely small quantities of discolourised fibrin.

Lungs anæmic anteriorly, especially the right; posteriorly, almost airless, and filled with dark blood, giving to both a dark purple colour.

Liver normal in appearance and size; gall bladder filled with thin bile, serous in character.

Spleen enlarged, weighing 15 ozs., and in a condition of putridity.

On opening the intestines it was found that from about the

commencement of the ileum the solitary glands were considerably enlarged, and, in some, distinct evidences of infiltration were observed. Where the infiltration was marked the gland was the size of a small pea, and had a yellowish appearance. The patches of Peyer were considerably swollen, infiltrated, and the enlargement was progressively marked towards the ileo-cæcal valve. The patches presented, in a typical degree, the appearances noted in the beginning of the second week of typhoid fever. There was no trace of necrosis or ulceration.

The cæcum and upper part of colon presented spots of intense hyperæmia, which gave a bright variegated appearance to the mucous membrane. In the centre of the spots were enlarged glands, some of which were ulcerated.

The mesenteric glands were considerably swollen.

As the case was an important one in many respects, I took some trouble in investigating the mode of development of the patient's illness. I was told by his employer that he had been complaining, for some three weeks before he took seriously ill, of occasional headache, and pains about his ankles. He dined, however, very well on Tuesday, the 6th November, but his appetite seemed to fail gradually until Sunday, the 11th November, when he expressed himself as feeling much better. Next day he was very ill, and on the day following he was seen by Dr. M'Evoy, who writes to me that, when he saw the patient, he found him complaining of frontal headache, urgent diarrhœa, and some cough. His pulse was 100, full and soft; temp. 103° F. There was a good deal of tympanitic distension of the abdomen, so much so that splenic enlargement was not determinable. The ileo-cæcal region was tender on pressure, and over the abdomen six or eight spots of the usual roseolar character of typhoid were observed.

Taking the 12th November, from the acuteness of symptoms present, as the date of invasion of typhus, it is to be noted that the typhus rash appeared upon the 17th November, or fifth day of the disease, and that death took place on the twelfth day of the disease.

I assume there can be no doubt regarding the nature of the fever to which the patient succumbed, so the point to be determined in the case is whether the fever was a simple case of typhus with

implications of the lymphatic structures in the intestines, or that the case represents a mixed form of disease—typhus grafted upon a case of typhoid fever.

The most important evidence as to intestinal implication in typhus is afforded by examination of a few rare cases referred to by Liebert as occurring in what is known as the Breslau epidemic, and the cases recorded in the well-known essay of M. Landouzy. In the former, the solitary glands and Peyer's patches, usually in the vicinity of the ileo-cæcal valve, were the seat of small isolated spherical ulcers, but there appeared to be an absence of medullary infiltration in the cases, so as to suggest that the condition found was merely an exaggerated catarrh of the intestine, such as is met with in various acute diseases.

M. Landouzy describes the breaking out of typhus in the jail at Rheims, during which 138 persons were attacked, of whom seventeen died. Six *post-mortem* examinations were made, but of these two only are recorded. We are told that in one case the intestines were examined by M. Chabaud and two other physicians, and pronounced to be sound; then M. Landouzy, entering and re-examining the specimen, detected what he considered evidence of incipient changes in Peyer's patches with general slight tumefaction of the solitary glands. The second case recorded would appear to be really one of typhoid fever. There are no strong points in it to support the diagnosis of typhus which was made. Typhoid deposit was found in Peyer's patches, and the case may be regarded as one of typhoid—a view that is supported by the fact that typhoid was prevalent in the prison during the time of its occurrence along with the existence of an epidemic of typhus. Even if it be thought by some that in the first case of M. Landouzy the intestinal lesions were of a sufficiently marked character, there is nothing to negative the view that the two fevers may have been present at the same time.

In examining the intestinal lesion present in the case submitted, there is a point which needs some comment. The lesions found do not correspond chronologically to the ordinary progress of a typhoid lesion. But we should not overlook the fact that,

in a certain proportion of cases, the medullary infiltration of the solitary glands and Peyer's patches does not proceed to the necrotic stage, but passes through retrograde changes, until by degeneration and absorption the infiltrated parts return to their normal condition. This is probably what occurs in the mild and abortive forms of typhoid fever. Furthermore, we may be inclined to admit that the influence of one profound condition of general infection, such as typhus, might modify or arrest the progress of lesions produced by a second form of general infection, such as that due to the typhoid poison. We have certainly instances of acute diseases modifying the course and progress of other affections with which they are associated.

Before stating the points which influence me in regarding the case brought before you as one of co-existing typhus and typhoid fever, I do not desire to appear in any way as impugning the view that intestinal lesions, similar in character to those met with in typhoid, occur under a variety of pathological conditions. I am, indeed, incorrect in referring to this as a view; it is much more—it is a fact repeatedly demonstrated. In scarlatina, measles, small-pox, and cholera, such lesions are met with. The condition known as psorenterie has been found as a frequent accompaniment of cases of scarlatina, running a very rapid course, with high temperature. Here the lesion of the lymphatic structures in the intestines is mainly to be found in the solitary glands, the patches of Peyer being usually more distinct than normal, but not enlarged to a degree corresponding to the swelling of the solitary glands. Such, at least, was the character of the intestinal lesions in two cases of psorenterie observed by myself. Why there should be any distinction between the condition of the solitary and the agminated glands, it may be difficult to understand; but we must bear in mind that Cruveilhier described a form of typhoid where the lesions were altogether confined to the solitary glands, the patches of Peyer remaining intact. This is the variety designated by him *forme pustuleuse.* I should be disposed to go even further, and admit the existence of the form of fever, described by Wunderlich and Niemeyer, where symptoms and lesions, almost identical with those

of typhoid, are produced, not by a general infection, but from a purely local cause. I look upon the absence of such a fever—gastro-enteric in its nature—from our classification as an omission, inasmuch as it leaves out of consideration those cases so familiar to physicians, which stand between febricula and typhoid, to which the much-sneered-at term "gastric" is given. The mere existence of identical anatomical appearances in two cases of disease does not necessarily imply identity of disease. The symptoms and morbid anatomical appearances met with in cholera nostras may be perfectly indistinguishable from cholera Asiatica. Precisely similar conditions of throat may be found in scarlatina and diphtheria, the only distinction between the two being the existence in the latter of the micro-organisms discovered by Löffler.

Without further occupying the attention of the Section, I shall briefly tabulate the points in the case submitted which afford evidence of the existence of typhoid, and which, from their prominence, led, at first, to a diagnosis of this disease.

Typhoid physiognomy.

Rose-spots sparsely scattered over chest and abodmen.

Early enlargement of spleen.

Epistaxis and

Persistent diarrhœa.

The *post mortem* appearances disclose medullary infiltration of the solitary glands and patches of Peyer, progressive towards the ileo-cæcal valve, presenting the appearances described as the *plaques reticulées* and *plaques gauffrées* of Chomel, and accompanied by swelling of the mesenteric glands. The solitary glands in the cæcum and adjoining part of the colon are found enlarged, infiltrated, and, in some parts, ulcerated. The spleen is greatly enlarged, softened, and in a condition of putrilage.

The case occurs during the marked prevalence of typhoid fever. Add to these the weight of authority.

Liebermeister lays down: "The lesions we find in the body after death from typhoid fever are so characteristic that they of themselves make known the nature of the disease, even if we were ignorant of the symptoms and etiology of any particular case.

This is not only true of more severe cases, but even in very mild cases, where death results from some complication. An experienced observer can find enough lesions to make the anatomical diagnosis."

I may also refer to the brief note of a case of typhus, complicated with diarrhœa, which appears in the "Transactions of the Pathological Society of London," Vol. XVI. It bears upon mine in this way, that in typhus, with symptoms of intestinal irritation, where one would expect to find signs at least of intestinal catarrh, these signs were completely absent.

The patient, a man aged forty-four, had passed through an attack of typhus, complicated with diarrhœa. On the 13th day of the fever the pulse had fallen to 84, and the rash was fading, but the diarrhœa continued obstinate, and the patient gradually becoming weaker, died on the 18th day from the commencement of his illness.

In this case neither Peyer's patches nor the solitary glands of the ileum were enlarged, nor was there the least trace of congestion or ulceration in any part of either large or small bowel. Dr. Murchison, in his remarks upon the case, called attention to the frequency with which diarrhœa was met with as a complication of typhus; but in every instance where he had an opportunity of examining the intestines after death, no traces of any disease were observed.

Amidst the hundreds of autopsies of typhus recorded by Gerrard and Pennock, Stewart, Reid, Peacock, Jenner, Wilks, Jacquot, Godellier, Barralier, Murchison, and Lebert, not one case is recorded which corresponds, as regards the existence of intestinal lesions, with the case submitted. The observations of all those observers go to show that in typhus fever the solitary glands and Peyer's patches are more healthy in appearance than they are found in the majority of other diseases.

In arriving at the conclusion that the case brought before you to-night illustrates the co-existence of typhus and typhoid fever, I cannot but regard myself as fortunate in having the opportunity of demonstrating the occurrence of what is regarded by many physicians as problematical.

DISEASE

Complications

NAME

AGE

DIET

NOTES

Case Book N°

Date of Admission

Result

| MONTH & DATE. | | |

NOTE ON AN IMPROVED CLINICAL CHART.

By ROBERT GLASGOW PATTESON, M.B., &c.

[Read in the Section of Medicine, January 25, 1889.]

The clinical chart which I wish to submit to the members of the Academy to-night is one which is designed to meet some of the drawbacks which are common to most of those at present in use. The smallness of the charts now used in many of our hospitals renders it almost impossible for students, at any distance from the bedside, to see the daily fluctuations of temperature, pulse, and respiration, and thus one of the most valuable features of clinical teaching is rendered useless. By increasing the size of the chart, without, however, rendering it inconveniently large; by adopting a bolder and clearer style of printing, and by marking the mean temperature of the body, in health, with a distinct red line, it is hoped that these disadvantages have been removed. Again, the daily cultivation of the faculties of sight and touch, which is rendered necessary by filling up, in a regular and methodic way, the variations of the subjective and objective symptoms of disease, is one that cannot fail to be of advantage to the student from an educational point of view; and I venture to believe that an intelligent use of such a chart in hospitals would considerably lighten the teacher's labours, and would prove of immense value in systematising the students' methods of observation—the advantage of which, in after-life, could hardly be over-estimated.

On looking at the chart, you will observe that, besides the ordinary line for recording the frequency of the pulse, a second is added for the observation of its volume, suddenness, intermission, irregularity, dicrotism, or other characters. So, besides the rate of respiration, shallowness, prolonged expiration, Cheyne-Stokes', or other phenomena may be recorded. Next comes

a group dealing with the gastro-intestinal tract. First, the condition of the tongue, furring, dryness, fissures, &c., may be noticed; then the abdomen, under which distension or retraction, pain or tenderness on pressure, would naturally suggest observation; and lastly, the bowels, under which, besides the number of motions, the consistence of the fæces, the occurrence of blood, mucus, tenesmus, &c., would find a record. Space is next provided for a full analysis of the urine, abnormal ingredients being grouped under one heading. A special line calls attention to the quantity of urine passed daily, a most important question in disease, and one too often wholly neglected. Spaces for the condition of the skin, such as abnormal dryness, sweating, eruption; and for the occurrence of nervous symptoms, headache, the various forms of delirium, or the presence of paralysis, complete the chart.

I need not occupy time in suggesting abbreviations or symbols for filling up the chart, as these will naturally occur to everyone. With a little practice it can be very rapidly done, and the gain in accuracy and information will surely counterbalance the slight extra trouble involved. Without claiming for the chart the title which the publisher has been kind enough to bestow on it, I trust it will meet with the approval of the members of the Academy, and at least be found a move in the right direction towards a simple, accurate, and graphic method of recording a case. In conclusion, I have only to add that I am indebted to Dr. Purser for much kind criticism and suggestion: indeed, to him many of the most valuable features of the chart are due.

A METHOD OF GENERATING NEUTRAL FUMES OF AMMONIUM CHLORIDE OR BROMIDE FOR INHALATION; WITH DEMONSTRATION OF THE INSTRUMENT.

By PATRICK WILLIAM MAXWELL, M.D. Edin.;
Ophthalmic Surgeon, Jervis-street Hospital;
Assistant-Surgeon, National Eye and Ear Infirmary, Dublin.

[Read in the Section of Medicine, January 25, 1889.]

It is not so much my intention in this paper to discuss the merits of ammonium chloride as a local application to the mucous membrane, as to consider how it can best be applied. Most physicians are already agreed as to its efficacy in suitable cases. Some give it internally, whilst others apply it directly to the surface which it is intended to influence either as a wash or spray, or in the form of fumes. My own experience of the drug has been confined to catarrhal conditions of the nose and pharynx. I have found it most useful in cases where the secretion is scanty or nearly absent, and also in those where, though abundant, it is tenacious and difficult to dislodge. In such cases the greatest immediate benefit seems to be produced by a warm solution of the salt used as a nose wash. But I am inclined to think that, where repeated applications are necessary, the fumes produce, in the long run, the most good. Physicians, as a rule, prescribe the inhalation of the fumes, and certainly among patients this mode of exhibition is much preferred as being less disagreeable.

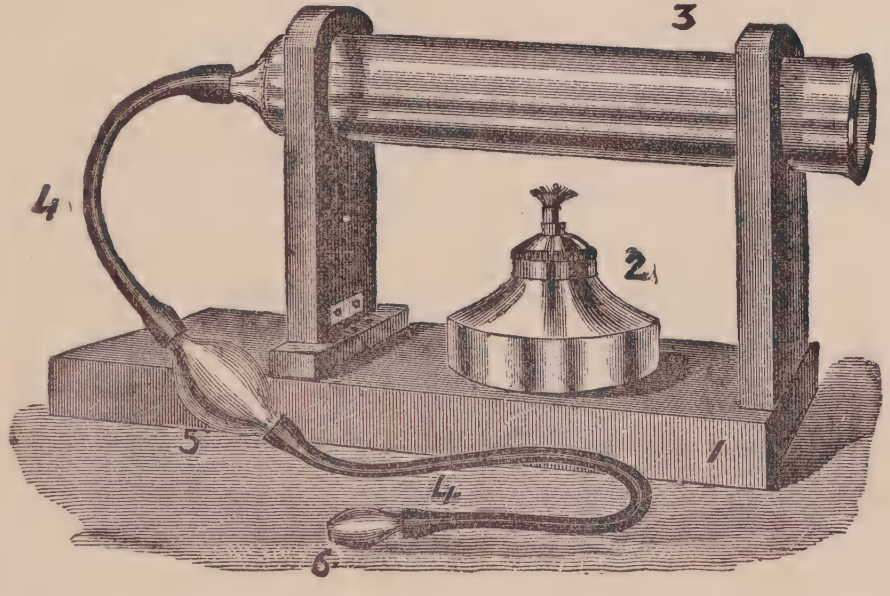
Various forms of apparatus have been devised for generating these fumes, but so far as I am aware the principle in all has been the same—viz., to obtain the vapours of hydrochloric acid and ammonia by drawing air through a solution of each, and then allowing these to mix. Minute particles of ammonium chloride

are thus formed and remain suspended in the air. When these so-called fumes are inhaled, particles are deposited on the surfaces with which they come in contact or become dissolved in the secretions. Nothing could be better than this method, provided the vapours of hydrochloric acid and ammonia could be supplied in the proper proportions. In practice, however, this is seldom obtained. The ammonia being the more volatile is in excess at the beginning of the process, and being more quickly exhausted allows the acid to be in excess later on. By passing the combined fumes through a wash-bottle, this tendency is to a great extent corrected. The preliminary excess of ammonia dissolves in the water, and the solution so formed will absorb a good deal of the free acid which finally comes over. That this is not completely effected is proved by drawing the washed fumes through another bottle containing litmus solution, which usually gives a decided alkaline reaction. A proper mixing of acid and ammonia is still further interfered with by the fumes passing back into the tubes leading from the original solutions and causing a deposit of the solid salt upon their walls. One of the tubes may thus become obstructed, and the fumes from the other bottle are then inhaled alone. An excess of acid is very irritating, and is liable to aggravate the condition which the ammonium chloride is intended to relieve. An excess of ammonia does not so much matter, and in some cases is thought to be even an advantage; still, when this happens, it cannot be said that pure ammonium chloride is being inhaled, nor can one be certain whether the free ammonia, even if desired, is in excess or not of what is intended. In dry catarrh an excess of ammonia will produce a greater immediate moisture, but is apt later on to increase the dryness.

At my suggestion Messrs. Anderson & Adams (68 Grafton-street, Dublin) have produced a very simple apparatus whereby solid ammonium chloride is volatilised by heat, and after mixing with cold air can be inhaled.

The apparatus consists of a wide piece of glass tube, like the cylinder of an ordinary glass syringe, placed horizontally on two wooden uprights. About 15 grs. of ammonium chloride are intro-

duced by a spoon through the wide end of this tube, and are placed in a little heap in the centre. The base is then lightly stopped with cotton wool. A lighted spirit-lamp, placed below the tube, volatilises the salt. A piece of india-rubber tubing with a glass mouthpiece is attached to the narrow end of the tube, by means of which the fumes are inhaled. A current of cold air rushing in by the large end of the horizontal tube mixes with the fumes and reduces their temperature, so that they are not more than slightly warm. These fumes are perfectly neutral from first to last, as is proved by passing them through red or blue litmus solution. Even phenol-phthalein does not become coloured. There is also no free chlorine, for starch paper is unaffected. In the course of the inhaling tube is placed a glass bulb filled with glass wool moistened with water. This prevents the fumes from being too dry. Besides being perfectly neutral the fumes supplied by this instrument have the advantage of containing more ammonium chloride than is contained in the same volume of fumes from the ordinary inhalers.



1. Wooden stand with hinged uprights, which can fold down for convenience in packing.
2. Spirit-lamp.
3. Glass cylinder for generating fumes.
4. India-rubber tube.
5. Bulb filled with glass wool.
6. Mouthpiece.

When it is desired to apply the fumes to the middle ear a tube rather longer than the ordinary inhaling one can be attached to a Eustachian catheter, and the fumes pumped in by an india-rubber handball fitted to the larger end of the horizontal tube.

The fumes, if desired, can be medicated by ol. pin. sylvest., eucalyptus, &c., by dropping about 5m of the oil upon the wool at the wide end of the tube.

As the iodine and bromine salts seem, when given internally, to have a greater selective affinity for the nose and throat than the chlorides, it occurred to me to try whether their ammonium compounds could be volatilised for inhalation. In the case of iodide of ammonium I found that though the greater part was volatilised as such, still it was mixed with so much free iodine and ammonia as to be very irritating and quite unfit for inhalation.

In the case of bromide of ammonium, when only a moderate heat is used, the salt volatilises unchanged, but as the temperature rises the odour of bromine can easily be detected in the fumes. Its presence can also be shown by its blueing starch and iodide paper. It was natural to expect, at least, an equal quantity of free ammonia. Red litmus was not changed, but phenol-phthalein gave a slight red. Ammonia is therefore present, but only in very small quantity. The quantity of free bromine must also be very small, as none of the patients who have as yet used these fumes have found them irritating.

It might, nevertheless, be advisable to get rid of the bromine. The wet glass wool absorbs all the free ammonia, but allows some free bromine to pass. The caustic alkalies would absorb the bromine, but would also decompose the fumes liberating the ammonia. I asked the advice of Dr. N. M. Falkiner in this matter. He said that as resorcin absorbed iodine it would be likely to have a similar effect on bromine. To try this I added a few crystals of resorcin to half a test tubeful of bromine water. Before doing so the water bleached litmus and blued starch and iodide paper. After the addition of resorcin it did neither. The solution changed from a pale yellow to a dark reddish-brown, and acquired a somewhat ethereal odour.

If the glass wool is wet with an aqueous solution of resorcin of about 20 grs. to the ℥ i., all free bromine will be absorbed from the fumes. All ammonia is also, as already stated, absorbed by the water, so the fumes may be said to be neutral and pure.

The action of bromide of ammonium fumes seems to be very similar to that of the chloride. The bromide seems to draw more fluid from the mucous membrane, its greater osmotic effect being due probably to its greater molecular weight; it seems to have a sedative action on the mucous membrane of the entire nose and throat. This has been observed for some time as regards the pharynx.

As the internal administration of the bromides is occasionally useful in tinnitus, I have thought it worth while trying its effect locally by blowing the fumes into the middle ear. Of course I do not suppose that this would benefit tinnitus of nervous origin, but it might be useful where congestion of the middle ear was the cause. As yet I cannot speak positively as to its effect in this way. I have, however, at present under treatment three cases of chronic catarrh of the middle ear with tinnitus which have resisted all the usual remedies. Each of these patients experiences a temporary diminution in tinnitus after applying the fumes through the catheter.[a]

The strongest hydrobromic acid (33 per cent.) gives off no vapour. I failed to produce any fumes by drawing air through such a solution and mixing it with ammonia. It is, therefore, evident that the ordinary apparatus for producing the chloride would be of no use for generating the bromide of ammonium.

[a] The condition of one patient remains unchanged. The other two are much improved (Feb. 16th).

THE DISAPPEARANCE OF CARDIAC MURMURS.

By M. A. BOYD, M.D.;
Physician, Mater Misericordiæ Hospital.

[Read in the Medical Section, January 25, 1889.]

In drawing attention to the disappearance of cardiac murmurs, I wish my remarks to be understood to have reference to the murmurs of organic endocardial origin. I purposely exclude all murmurs of pericardial or exocardial origin, which, as all practical physicians are aware, are the forms of so-called murmurs which are prone to disappear; and also all those of endocardial origin known as functional or anæmic murmurs, which are equally fugitive in character. I know many authorities hold that an organic murmur produced either by changes in the valves, or of the orifices they close, or of the appendages attached to them, once established, is never cured, but few of them venture to assert dogmatically that these murmurs never disappear as time goes on. Their disappearance, temporarily, from constitutional causes, or from alteration in the cardiac walls themselves, is not even regarded by many authorities as a proof of their absolute cure, and this applies especially to murmurs produced by chronic endocarditis, as opposed to those produced or appearing in the course of an acute attack. In fact, a well-established murmur produced from chronic endocarditis engaging an orifice disappears, according to such authorities, only during the time preceding dissolution, and, under ordinary circumstances, is always permanent.

In acute endocarditis, however, we often find murmurs become developed from organic changes in the valves, or the orifices they guard, leading to corresponding changes in the cavities of the heart, indicating thereby their organic nature, which frequently disappear after weeks or months of undoubted existence; and as

some cases of this character have come under my notice, I beg to draw attention to them. I may, in general, state that the murmurs produced during acute endocarditis that engage an orifice or its valves I have met with as disappearing in this way, have all been those associated with regurgitation, and I cannot say that I have met with an instance of a murmur due to constriction of any orifice disappearing once the symptoms indicating this condition have become undoubtedly developed. The reason of constrictive murmurs so seldom disappearing is obvious. The constrictive murmur in its pronounced form is generally a late condition in many instances of the mischief that led to or produced regurgitation from inflammation of the valves, or their attachments, or of the walls of the ostium in their neighbourhood. We all know how frequently constrictive murmurs of both mitral and aortic orifices follow from previously-existing regurgitative ones.

Many will, I know, meet my assertion that murmurs of regurgitation (undoubtedly organic) do disappear, with the remark that these murmurs are not always due to organic changes either in the valves or orifices, and are frequently the result of inadequate adaptation, or of atonic muscular action, or of alteration in the cavities of the ventricles with which they are associated. I grant all this, and while recognising the fact that regurgitation at any orifice may be due to a variety of causes, both constitutional as well as local, a number of cases exhibiting all the symptoms of undoubted permanency in character make their appearance during an acute rheumatic attack, or subsequent to it, and yet ultimately disappear, leaving the heart as free from traces of their presence as the lung would be free from any traces of disease after an attack of uncomplicated croupous pneumonia. Such cases are not by any means few, and most of them occur in connection with the mitral orifice; but I have also met with cases of aortic regurgitation where a permanent disappearance of the murmurs took place. I have also come across cases of tricuspid regurgitation where the murmur disappeared, to return again in course of time, so that I cannot adduce these as examples of permanent disappearance.

We are all familiar with the condition of disappearance of mitral

systolic murmurs occurring during the course of acute rheumatism, or of recurring rheumatic attacks, which show no trace of their existence, when, the temperature reaching its normal standard, and the arthritic symptoms disappearing, the general health becomes established, and this would be borne out by the prevalence in medical literature of the expression, "A slight blowing systolic murmur was present," showing the significance attached to such a murmur in the mind of the writer, as if it were one of no consequence, and likely to disappear. The general opinion, I think, with regard to mitral regurgitative murmurs in acute rheumatism is that they generally get well, and their disappearance in at least 15 per cent. of the cases so attacked is not too high a percentage of recovery to assign to them.

In looking over the literature of cardiac disease, one is struck by the meagre details furnished as to the ultimate disappearance of murmurs once established; and in no authority on the heart that I consulted, did I find any statistics on the subject, most of them directing their attention apparently to well-established murmurs from chronic endocarditis. Allusion is made by some to the causes of disappearance especially of mitral regurgitative ones during the course of a rheumatic attack. Niemeyer especially assigns the origin of those that disappear during rheumatism to "ataxy of the papillary muscles," and Rosenstein to the "failure of the muscular wall towards the close of life," and, if his theory is correct "that the intensity of the blood current is the cause of the intensity of the murmur and of its production," we can understand how a failing ventricle would be incapable of producing a murmur at an orifice where regurgitation was permitted.

Mitral murmurs should disappear, speaking theoretically, more frequently than aortic ones, owing to their being almost entirely the result of rheumatic endocarditis, which tends to get well, and not, as in the majority of aortic cases, the result of diseases of advanced life, such as atheroma, alcoholism, or gout—causes which do not disappear as age advances.

Mitral disease shows itself most frequently in the young, aortic in advanced life.

In the cases of disappearance of murmurs of mitral insufficiency that I have met, the subjects were all young, and as one case which I met some years since made an impression on me I will give the particulars of it :—

Case.—E. S., a young girl, aged twelve, suffering from pain and swelling in the region of left hip joint, with febrile symptoms, and apparent lengthening in the limb of that side, was regarded by me as developing the early symptoms of morbus coxæ. Leeches applied to the neighbourhood of the joint, with counter-irritation afterwards, relieved the pain, and all further trouble referable to hip disappeared. She then developed symptoms of pericarditis without showing any symptoms of further arthritic engagement. I then learned there were some symptoms of rheumatism in the family, though both father and mother were free from it. The pericardial attack, accompanied by lymphy exudation, ran its course, and at the end of the attack I was for the first time able to detect the presence of a mitral systolic murmur, which continued all through until convalescence was established and all febrile symptoms had disappeared. The girl was allowed to go about, with the usual precautions as to exercise. At the end of a few months I was again in attendance on her for a recurring attack of rheumatism ; this time with engagements of the joints, and evidently fresh endocardiac symptoms, with a pronounced mitral regurgitative murmur still present. She recovered from this attack with a hypertrophied left ventricle, and suffered afterwards from dyspnœa on the slightest exertion. As I looked on the murmur, from the alteration in the ventricle and the dyspnœa present, as an organic mitral murmur, I gave an unfavourable opinion to the friends as to the possibility of its getting better. This girl was from time to time under my care for the next two years, and I satisfied myself on each occasion I saw her of the undoubted presence of the mitral insufficiency, and of the hypertrophy of the left ventricle being present. I then lost sight of the case, notes of which I had carefully taken at the time, and I heard from the parents she was enjoying good health at school, that menstruation had come on, and that she ceased to complain of all cardiac troubles. On her return I was anxious to ascertain the condition of her heart, and she was brought to me for examination. I was surprised to find all traces of murmur and hypertrophy had disappeared, and she was able to go through any amount of exertion without the slightest dyspnœa, and I satisfied

myself of this by repeated examination. Since then I have met with similar cases amongst hospital patients, all occurring in young subjects.

In what manner can we explain the disappearance of such organic murmurs? Hayden, in his work on "Diseases of the Heart," ascribes the murmurs of mitral regurgitation, besides those due to valvulitis with thickening and interstitial deposit, to the following causes:— "The mitral valve," he says, "may, however, be incompetent without having undergone any change of structure whatsoever:—(*a.*) Valvular inadequacy and regurgitation from displacement of one or both segments of the valve through the papillary muscles and tendinous chords, by excentric yieldings of the walls of the left ventricle at the acme of systole. This result may be due to fatty degeneration of the left ventricle. It may be likewise attributable to debility without structural alteration of the valves, or of the ventricle from anæmia, malnutrition, or asthenic disease. (*b.*) Regurgitation from rupture of one or more of the primary tendinous chords. (*c.*) From dilatation of the left ventricle without proportionate elongation of the papillary muscle. (*d.*) Weakness and relaxation of the papillary muscle. (*e.*) Adhesion of the tendinous chords to the wall of the ventricle. (*f.*) Engagement in the orifice of a pedunculated mass of fibrine depending from its attachment to one of the curtains of the valve or to the interior of the auricle. (*g.*) Fixation of the anterior segment of the mitral valve by a calcareous mass descending from the aortic orifice and attached to the valve.

Under which of these heads can I include the case just mentioned with others of a similar nature? It would seem to me to belong to that of thickening with interstitial deposit, which must have, in the course of time when competency became fully established, got absorbed, as other deposits of a similar nature during the rapid metabolic changes that take place in a growing subject or one nearing maturity.

On this point Hayden again says—"In the opinion of some writers a process of tissue-change in the direction of cure may be set up; the hypertrophied connective tissue of the valves being

reduced to its normal proportions by absorption, and the deposit, if any, disintegrated and detached by the blood current, to be subsequently dissolved and excreted. I cannot," he says, "subscribe to this doctrine, inviting though it be. The valves of the heart are not so circumstanced as to admit of this favourable change. They are in the performance of their functions subject to the pernicious influence of three forces operating alternately, namely—motion, friction, and tension, which are eminently calculated to maintain in them a state of constant irritation or chronic endo-carditis, necessarily resulting in progressive valvular disorganisation. This downward progress once set up may be restrained or retarded, but not arrested or reversed." With the greatest respect for the opinions of such a high authority, I think this assertion is too dogmatic and not borne out by the clinical experience of others.

We should be less prepared to find the murmur of aortic regurgitation disappear once such a condition becomes apparently permanent; but I have met with two such cases—one in a gentleman aged 45, the other in a girl aged 20. In the gentleman's case I only discovered the presence of the murmur when giving him an opinion as to whether he would be taken as a good life for assurance. I found at my examination then that the left ventricle was dilated, but moderate hypertrophy was present also, and as the patient made very little complaint referable to his heart or to symptoms depending on regurgitation, I concluded the case was recent. I told him I did not think an insurance company would accept him owing to an affection of his heart which I found present. As the patient was addicted to the free use of alcohol I cautioned him against it, as I could not find any other cause for his valvular affection, he being neither gouty, syphilitic, nor albuminuric. I saw him again a month later; the murmur was still present, but there was no further increase in either the hypertrophy or dilatation, and he only complained of occasional sudden perspirations troubling him at night. After a further month's treatment he returned with the regurgitation cured, as the murmur had entirely disappeared. In the other case, that of the young girl, the aortic regurgitative murmur arose out of an attack of acute rheumatism,

and continued with symptoms of dilatation and hypertrophy of the ventricle for over two years. A recurrent attack of rheumatism then came on with pericarditis, and after it had passed away, instead of the heart being worse and the aortic regurgitation increased, the murmur had disappeared, and she has had no return of rheumatism or cardiac troubles since, though several year shave elapsed.[a]

I cannot offer an explanation of how these murmurs have disappeared, unless by the absorption of plastic material deposited on the edges, or in the substance of a valve, and preventing adaptation; no symptoms of constriction existed in either case.

There is one practical lesson, at all events, I have learned from observing murmurs arising in the course of rheumatic endocarditis—never to pronounce a patient the subject of incurable cardiac disease for at least a month or two after convalescence, and I am more disposed to lengthen the time before delivering this opinion than to curtail it.

[a] Since writing the above, Dr. D. Campbell Black, of the Royal Infirmary, Glasgow, has kindly called my attention to a case of double aortic murmur reported by him in the *British Medical Journal*, of February 16th, 1884, which entirely disappeared, the patient, he adds, being perfectly free from all trace of disease at the present time.

TENERIFFE AS A HEALTH-RESORT.

By J. K. BARTON, F.R.C.S.;
Surgeon to the Adelaide Hospital.

[Read in the Section of Medicine, March 1, 1889.]

The Canary Islands, and specially Teneriffe, have lately come so prominently before the public as health-resorts, that it occurred to me that the members of the Medical Section of the Academy would be interested to hear from me some account of a visit which I paid to that island last spring.

I may begin by stating that the entire time that I was absent from Dublin was not quite four weeks; ten days were spent in the island, so that the journey there and back was a day or so more than a fortnight.

Steamers start from London and Plymouth, and from Liverpool—those from the latter port (Liverpool) take longer and are not as good vessels as the London services. The fare is less, being only £15 for a return ticket, whereas it is £25 by the London vessels; but for an invalid the London boats are to be recommended, for they are better found in every way. They are advertised to make the passage in seven days from London, or five from Plymouth to Santa Cruz. The vessels of the "Shaw-Saville" Line are splendid boats, and the accommodation is very good, but they are slow, and the seven days is apt to become nine, or the five seven from Plymouth. We went on board a fine vessel belonging to this Company (The Doric) in the Thames, opposite Tilbury Fort, at 1 o'clock on Thursday, the 22nd of March last, and sailed at 3 o'clock, but we did not reach Plymouth until Saturday morning at 2 a.m., and did not cast anchor in the harbour of Santa Cruz before 8 a.m., on Friday, the 30th, being Good Friday—that was *seven* days from the day we left Plymouth. I think this was unusually slow, and

that we may safely say to those we advise to make this journey that five or six days from Plymouth will suffice to bring them to Teneriffe. Nor, sailing at an earlier date, say in January, would they be likely to meet the rough weather which we encountered at the end of March, when equinoctial gales prevail. As we came down the Thames, and then along the south coast to make Plymouth, we encountered continuous fogs; and a constant thick drizzle of rain, with a heavy roll of the sea, rendered the beginning of the voyage rather dismal. It was a bright morning as the vessel lay in Plymouth roads, and all looked so cheery after the dreary two days we had had that we began to think all would be smooth and bright for the rest of our journey. This anticipation was soon dispelled, for on looking at my diary I find the following entry:—"Sunday, 25—A bad day, ship rolling fearfully; feel ill, went to bed at 12 o'clock; very sick all day." It was not until we were within sight of the "Peak" that the rolling began to abate and it became possible to walk the deck. As we approach Teneriffe from the north the view of the island is very fine. The cliffs seem to rise straight and dark right up from the sea, while round their base the white foam of the breakers contrasts with their sombre look, and high up in the clouds rises the Peak, its snow-covered summit glittering in the rays of the sun, and seeming even higher than it is, by reason of the belt of cloud which usually encircles the mountain somewhat lower down.

Passing round the headland to the east, we come in view of the harbour of Santa Cruz, where the trouble is not to get deep water for a big ship, but for the big ship to find bottom for her anchor. Like most volcanic islands, there is very deep water close up to the rocks.

It was Good Friday when we cast anchor and got on shore, and we were told that to drive across the island to Oratava on this day would not be allowed, but like many other difficulties this vanished at close quarters—the only deference which we were expected to pay to the sacred character of the day was that the carriages were not allowed into the town, where indeed they would have made a fearful clatter upon the rugged pavement, and so we had our goods

carried about half a mile to the outskirts, where the carriages were drawn up.

I may halt here a few minutes to describe Santa Cruz. Seen from the water the white flat-roofed houses show very prettily, stretching along the shore, while rising above them are the deeply-indented mountains covered with grass three-fourths of the way up, but the tops rugged and rocky, reminding us of their volcanic formation. Santa Cruz has a capital Spanish hotel, whose host treated us with such kindness that it must be told of him. On our return from Oratava the town was full, not a bed to be had, all waiting for the steamers. We were offered a room, from which we shrank back aghast—no windows, and the smell (faugh!). Under these circumstances, and night coming on, we again accosted Senor Comacho, who at once gave us his own room, clean and airy, on the *first* floor of his hotel, with every modern comfort, for three days; and when we left charged nothing more than the usual very moderate tariff. The start for Oratava was made about 10 a.m. Five or six carriages, with three or four wiry, active, half-bred, and entirely untrained horses being yoked to each. The start was accompanied with a fearful cracking of whips, and with much gesticulations and galloping, but this soon subsided, as the ascent became very steep. The distance from Santa Cruz to Lugano is only five miles, but here we are at the top of the ridge which divides the island, the watershed which descends rapidly on the Santa Cruz side, and very gradually on the opposite or Oratava side, it being fifteen miles from Lugano to Oratava. The first thing which attracted our notice as we toiled slowly up was the numerous cactus gardens. This supported a famous trade some years ago, that of *cochineal*, now nearly gone, as other dyes from America have quite, or nearly quite, displaced it in the market. The cactus flowers were all tied up in little bags of netting to preserve the cochineal insect, whose habitat this is, from being blown away or injured in other ways. Perhaps the next most remarkable object are the native men, who stand or walk leisurely along, clothed in a Manchester blanket, tied round the throat with a string; there is no sort of adornment, it is as common a blanket as can be, and looks

particularly hot and out of place, with a semi-tropical sun shining and the air balmy and warm. To work in such a garment is out of the question, so when the Teneriffer is compelled to do anything he lays his blanket aside, and then appears in short linen coat and breeches—a much more suitable get-up.

At Lugano the carriage rattled over the huge paving-stones of two or three most melancholy and deserted streets, and then drew up opposite as pretty, clean, and inviting a hotel as traveller could desire. We found it quite full; not a single bed to be had, and the courteous host, who speaks English fluently, told us that all his rooms are bespoken months before. As Lugano is considered the highlands, the bracing place to go to when the heat of the sun becomes oppressive at the sea-side, so now at the beginning of April it was quite full. While we rested in this enchanting place and mentally determined that we would some time or other stay there, we fell into conversation with a gentleman, who informed us that he had spent two winters here, and that the place had saved his life. He was a physician, practising in the south of England, who was attacked with symptoms of so violent a description as to be very nearly fatal. He was between sixty and seventy years of age. So closing his working life he sailed for the Canaries and had found at Lugano the exact elevation, air, and surroundings which suited him. His praise of *Lugano* was only equalled by his dislike to *Oratava*, where he informed us we should be starved—nothing fit to eat being ever placed on the table, while the rooms were filthy, and the air most relaxing. We endeavoured to cheer one another as we drove on by the reflection that if Oratava proved so very bad we could come back to Lugano and sleep in the coffee-room, but the bright sun, the balmy air, warm and yet fresh, the many varieties of trees and plants which attracted our attention as we drove along, dissipated any gloomy fear of starvation, and prepared us to enjoy the lovely views which opened upon our sight as we descended the broad Military-road which connects Santa Cruz and Oratava. The Peak comes into view soon after leaving Lugano, its sharp top looking out above the clouds; then gradually as we descend the mountain ridges from which the Peak springs come into view, and the

beautiful coast-line, which Humboldt described as the most beautiful *in the world*, and which, consequently, has disappointed everybody ever since.

The first village we pass is Matanga, where a capital midday dinner or lunch is spread, and after this, seven or eight miles further on, we pass the Villia Oratava, the Puerto or sea-port, lying much lower down on the coast. The two Oratavas are separated by about a distance of five miles and supplement one another—the Puerto being the more sheltered and much warmer, and therefore, for delicate persons, the better winter residence, while the Villia is more airy, and, at the time I visited the island in April, much the more agreeable atmosphere of the two, and I am certain the more health-giving air, for it stands high on the slope of the foot hills, which lead up to the peak, and is open on three sides, the fourth being closed by the mountains. Oratava (Puerto) was reached at 4 p.m., the twenty-five miles having been accomplished in six hours. This drive right across the island gives the traveller an excellent insight into the character of the country. Besides the points I have already noted, I may here mention that the volcanic origin of the island is very often demonstrated by the high hills or curious mounds of ashes, and everywhere the dry burnt lava-like stone projects from the cuttings. The vegetation is semi-tropical, and yet the island is not nearly as green as our own; ashes and piled up masses of volcanic stones constantly presenting themselves amongst the green of grass or trees.

The road from the Villia to the Port Oratava is a first-rate piece of engineering, being an easy slope all the way, planted on both sides with blue gum trees.

Arrived at Oratava we were at our head-quarters—having obtained rooms in the Grand Hotel and Sanatorium, which high-sounding title is after all not so absurd as is usually the case—for here very fair accommodation, with civility, and moderately good food are provided; but of this more particularly by-and-bye.

The special points upon which we need information regarding any place to which we are invited to send patients who require a winter health-resort are—1st, climate; 2nd, accommodation; 3rd,

surroundings, including amusements and company; 4th, facilities of coming and going. Of these the first stands forward as *the* essential, without which all the others are useless, and with which they are useful accessories. Now, the climate of Teneriffe is just that which one can speak of with perfect satisfaction. I have heard it extolled in the most exaggerated terms, which is a pity, for without being perfect, it is almost absolutely perfect for a great part of the year. First I will describe how I found it during ten days in April of last year, then I will tell you what I heard from numerous invalids and others who had spent the winter there, and lastly, I will give you the report kindly sent to me by Dr. Wharry, a young English doctor who has lately settled in Teneriffe.

1st. Of my own experience. We had sunshine every day, but not all of every day; twice in ten days we had rain; the very first day we spent in Oratava we got a good wetting; and two or three days afterwards we had a wet morning up to 1 or 2 o'clock. The air was under all conditions most balmy and agreeable, very warm but not hot. In the middle of the day, when a semi-tropical sun was shining, there was always a fresh cool breeze from the sea which took away all sense of fatigue. So that, on the whole, my short experience induces me to say that the climate is delightful, and, although so greatly lauded, that it has not been over-praised. 2nd. As to the reports from those who had spent the winter there. An elderly clergyman, who had been there since the previous October, said—"I have been laid up with bronchitis for several winters at home; I caught cold, and it invariably got to my lungs, and I spent six weeks in my bedroom and three months in the house. I came here in October. I remained perfectly well until January, then I caught a cold—but the difference was that I was never a day in bed, or in the house either—the air was so balmy that I was out every day, and the cold did not engage my lungs." Others gave the same evidence. "We have been out every day through the whole winter and have not taken cold; if a cold was taken it passed off in a few days, and never required us to keep away from the billiard-room, which was the favourite haunt of the Oratava Grand Hotel—being open completely to the air on

two sides—a lattice covered with the Bougonvillia creeper being the only protection from the outside weather."

In fact, whatever else the visitors might differ about and find fault with, they were all agreed about the weather and that the climate in winter was *delightful*.

3rd. Dr. Wharry's report. Dr. Wharry is a young Englishman who served in the Egyptian Army, and while doing so had an attack of hæmoptysis. He came to the Canaries with the idea of practising there, and being an educated man, and married to a most agreeable lady, he is an acquisition to the Island of Teneriffe. Any physician sending a patient there will be certain, by supplying him with a note of introduction to Dr. Wharry, that he will be carefully and very ably looked after. He begins by dividing the year into two parts—from December 1st to May 1st, and from May to December. English people coming out to Teneriffe in October, and therefore arriving in the summer half-year, should stay at Lugano until December 1st, when they should move down to Oratava, which will be the safest residence for them until May following, when they will begin to feel the relaxing effect of the air upon the coast, and will be much benefited by the higher air of Lugano, and if they stay the summer they should remain at Lugano, where the highest temperature reached in 1888 was 78·7°, and the lowest during the summer months 68·5°.

In Oratava what is called there "Indian Summer" prevails from July to November, during which the heat is excessive and the air saturated with moisture, the thermometer marking 82° F. During this period all patients should leave Oratava. Dr. Wharry mentions several cases of chest disease which remained all the year at Oratava, and fell off to an alarming degree during the months I have named.

There are no statistics as yet as to the effect of a prolonged residence in Teneriffe in cases of phthisis; but Dr. Wharry thinks that the higher air of Lugano is better suited for such cases, the lowest temperature recorded there being 58°. As a summer residence Lugano is the best in Teneriffe; its chief characteristics are a bright clear sky, combined with a dry bracing air and almost

total absence of rain. In winter it is subject to rain, cloud, and mist from the lofty neighbouring mountains.

At Oratava the mean temperature from November to May is 71°, never higher than 77°, nor lower than 56°—a variation of from 4° to 5° throughout the whole year shows a very remarkable equability of temperature, and this is one of the most marked characteristics of the climate, to which I may add that it contrasts very remarkably with our own. Dr. Wharry mentions two cases of spasmodic asthma placed under his care in Oratava, neither of which did well; and I would myself be inclined to say that for cases of phthisis, in which hæmoptysis was a prominent symptom, Teneriffe is not suitable—probably from the same cause which made the asthma cases do badly.

After climate comes "accommodation." This is at present very fair, and is rapidly improving. At Lugano the only complaint that can be fairly made of the hotel is that it is too small, and that it cannot receive one quarter of the people who desire to go there. Patients, however, who are residing for the winter at Oratava should engage rooms at Lugano months before. They have the advantage over all new comers, and, indeed, the rooms are always thus engaged. At Oratava there is much more extensive accommodation; the Grand Hotel has good rooms, a very fair cuisine, a billiard-room, and a verandah, which becomes the favourite lounge of all who reside there. There was a very general grumbling about the arrangement of meals, which was on the English plan; the climate and the idle life would make the French plan, of early coffee and a déjeuner at 11 o'clock, with dinner at 6 o'clock, much better. There are several Spanish hotels in Oratava besides the Grand, and private lodgings in abundance, where the visitor sleeps, taking his meals at the Grand. These are all very good, but a new hotel, with numerous detached villa houses, is about to be built on a capital site, considerably higher than the Grand, which is down on the shore. This may be open in a year from this time. At Acod I understand there is good accommodation also.

Now, as to amusements. The general feeling that prevails at

Oratava is to do *nothing*. All after a short time agree that by far the best thing to do is to idle about and enjoy the air; but for more energetic individuals one of the healthiest of all amusements is always to be had, and that is riding. The country is peculiarly suitable for this, as it is intersected in all directions with narrow paths and mountain roads, impassable for vehicles, but over which the native small, active little horses travel briskly; besides these there are a better class of horses, also small and very active, but well bred and handsome. These are Andalusian horses, and are always to be had at a few minutes' notice. Walking is rough and fatiguing, and so it comes to pass that riding is *the* daily enjoyment; but wherever young English people congregate there will be tennis and cricket, and in fact tennis courts form a part of the prospectus of the new hotel.

It only remains to say a few words of the best methods of getting to and returning from Teneriffe, as this has been much commented upon as being disagreeable, and worse than all *uncertain*, so that a week or more may be spent in waiting for a return steamer. Now, to all these, as well as to the rumour which reached us as to the food and accommodation, we found the difficulties and defects had been greatly exaggerated. I have already said the Shaw-Saville steamers are splendid boats. By the African Steamship Co. the passage is made from Liverpool in eight or nine days. I am told by those who have travelled by them that the accommodation and food are very good.

Now, as to the uncertainty of returning, I only heard of one party who had to wait a week, and this was from an accident which is not likely ever to occur again. As a rule there is room enough in the steamers returning from New Zealand, which all touch at Rio, from which a telegram is sent as they leave that port for Teneriffe, saying what day they left there and how many passengers they can take in at Santa Cruz. So that there is plenty of time to prepare; and if by chance this particular ship was full there are steamers passing almost every day, some French, some Spanish, some English, by which a passage can be obtained.

And now very briefly to sum up. The climate of Teneriffe is

dry, warm, equable, bracing in winter. In summer relaxing and hot at Oratava, when it is bracing, warm, and agreeable in the highest ground of Lugano. There is very fair accommodation, which is improving in quality, and extending very much so as to meet all demands.

There is abundant amusements and company for all, from the invalid who requires to be carried in the luxurious hammock, to the mountaineer who ascends the peak. And last, not least, there is a thorougly reliable, well-educated physician, Dr. Wharry.

SELECT CLINICAL REPORTS.

By C. J. NIXON, M.B., LL.D., Univ. Dubl.;
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[Read in the Section of Medicine, March 1, 1889.]

MYXŒDEMA.

As during a previous Session I had an opportunity of bringing under the notice of the Academy an instance of myxœdema, I shall only direct attention in the briefest manner to the points in the present case which identify it with the type of disease so fully described by Sir William Gull, Ord, and others. In diseases which may be regarded as comparatively new as far as their recognition is concerned, there is some need for recording cases with a view of establishing facts in reference to ætiology, pathology, and symptomatology. The patient exhibited is a married woman, aged forty-seven, a tailoress by occupation, and a native of Landau, in Bavaria. The district is a very mountainous one, but there is no prevalence of goitre in it. The patient's family history is satisfactory in all respects. Living in Ireland for the past 23 years, she enjoyed perfectly good health up to six years ago. She had not experienced any special privations; never suffered want, as regards either food or clothing, and had no occasion for mental distress. About six years ago she fainted one day whilst in church, and subsequently suffered from severe chill whilst walking homewards on a wintry night. From this time she never felt strong, and a year afterwards some swelling occurred in the upper part of the right cheek and right upper lid. The swelling, according to her statement, was confined to the right side for 12 months, when the left cheek and eyelid became affected. At this time she got very stout and bulky; her feet and legs were much swollen, and

she became unfit to undertake any active form of exercise. Menstruation was scanty but regular in its occurrence.

The symptoms noted since she came under my care in hospital are as follows:—

Shiny-white and pasty appearance of the face, with a faint circumscribed pinkish tint over malar bones. In front of right ear is a small patch, apparently of an ecchymotic nature, which has recently made its appearance. Very marked œdema of the upper eyelids, which present a waxy white aspect. The lids pit upon pressure, but not at all to the same extent as in the ordinary forms of œdema. There is considerable swelling of the lips and tongue, and the movements of the former are sluggish and clumsy, especially when the patient speaks. The alæ nasi have become widened out, and the patient states that her appearance is so altered that intimate friends have been unable to recognise her. The skin under the chin is loose and baggy.

The teeth are decayed, and there is constantly an extremely fœtid odour from the patient's breath, of which she does not appear to be sensible. The skin over the body is harsh, dry, and furfuraceous in most parts, and the hair in the axillæ and over the pubes is extremely thin. There is evidence of œdema in the legs and trunk; pitting on pressure can in parts be demonstrated. The movements of the patient in locomotion, like her method of speaking are slow and deliberate. She has always a feeling of chilliness over her, especially in her legs, and she complains of an absence of sensibility in the soles of the feet. Her temperature during her stay in the hospital has never been above 97° F., and on one occasion it fell below 96°. The feet and hands exhibit the harsh, dry condition of the skin noted in this disease, but the spade-like appearance of the latter described by Sir William Gull is scarcely marked.

The heart is normal as regards percussion, and the sounds, though weak, are free from murmur. The impulse beat is indistinct, but in the usual position. The pulse is very weak and compressible; it ranges between 56 and 72 beats in the minute. The sphygmogram exhibited shows its characters. The patient's mental

condition is one of extreme placidity. Even allowing for the *sang froid* of the German character, there is a remarkable degree of mental inertia about her. She has latterly become very drowsy, and it is with difficulty she can prevent herself from going asleep during the day. There is no affection of the special senses except that of hearing, which is somewhat impaired.

The urine has been repeatedly examined by the heat, nitric acid and picric acid tests. It presents no trace of albumen, and is free from tube casts. A careful examination of the blood and œdematous fluid was made for me by Dr. McWeeney, pathologist to the hospital. The fluid removed from the eyelids contained an appreciable amount of mucin; in the blood a moderate increase of the white cells was noted.

An examination of the patient's neck shows complete atrophy of the thyroid gland. Some remains of it may be felt crossing the trachea, but there is no trace of the lateral lobes. Above the clavicles, in the space between the sterno-mastoid and trapezei muscles, are to be observed soft, doughy swellings, apparently collections of adipose tissue.

Tabulating the symptoms existing in myxœdema, we find them to be as follows:—

Œdematous infiltration of cellular tissue, with a fluid rich in mucin.

Trophic disturbances in the skin and its appendages.

Sluggishness of movement, and a feeble and languid circulation, with low bodily temperature.

Slow processes of cerebration, and a placid and gentle temperament.

Atrophy of the thyroid body.

Probably the best way of describing the nature of myxœdema is to regard it as a trophoneurosis, originating in atrophy or removal of the thyroid gland. Its greater frequency in women than in men, irrespective of its occurrence as a result of operative procedure, when the ratio is the same in both sexes, appears to be connected with the close relation which the thyroid gland has to menstruation.

It is remarkable that two glands, both ductless and vascular, like the suprarenal capsule and the thyroid, should in disease produce conditions almost malignant in their course and effects. Disease in both involves material changes in the blood, and in both the connection of those changes with the primary lesion is by no means clear. Taking into consideration the structure of the medullary part of the suprarenal capsule, and its close relation with the semilunar ganglion, it is intelligible that an irritation propagated from it may be the initial starting-point of the profound nervous disturbance upon which the symptoms of suprarenal melasma depend. A like result not improbably follows the degeneration of the thyroid gland in myxœdema, or as a result of injury to the nerves in connection with the gland caused by its ablation.

It is remarkable, in reference to this point, the frequency with which tetany follows surgical removal of the thyroid body—viz., in 10 out of 68 cases operated on by Billroth, and in 3 out of 10 recorded by Reverdin. The nervous symptoms in the disease, and especially the mental state of the patients—a condition described by Reverdin as one in which the works of the machinery seem perfect, but the wheels are clogged with oil—indicate a widespread disorder of the central and peripheral nervous system.

SENILE CHOREA.

The notes taken of this case are briefly as follows. For many of the particulars I am indebted to Mr. Richard Smyth, my very zealous clinical clerk :—

Thomas M., ex-Constable, R.I.C., aged fifty-six, admitted into hospital on the 6th of Feb. last. He was dismissed from the Constabulary after a service of seventeen years for some irregularity in respect of a shilling, for which he could give no satisfactory explanation. This, and his inability to obtain employment, weighed heavily upon his mind, and he became depressed in spirits and irritable in temper. He had been through life a strong vigorous man, never suffered from rheumatism, and he gives no history of any nervous disease existing in his family.

Four years ago he complained, whilst working as a farm labourer, of weakness in his legs. This continued with some signs of abatement until two and a half years ago, when his wife died, and this event appears to have preyed upon him. Violent and irregular movements set in at first in the legs, and subsequently, after a lapse of about six months, in the arms. When the movements had been established some time in the arms, those in the legs gradually became less marked.

A month before admission he suffered much from insomnia, associated with a constant wriggling movement of the body whilst in bed.

The condition of the patient at present is as follows:—Choreiform movements of arms, head, and trunk, intensified upon emotion, excitement, or when the patient speaks. Characteristic mode of protruding and withdrawing tongue; occasionally a quick jerky method of speech. The gait is peculiar; when about to walk he stops for a moment, makes a sudden bend forward with his body as if he were going to fall forwards, sways violently from side to side, and then moves on. There appears to be some general muscular weakness, but no local paresis. The dynamometer registers a pressure with each hand of 47 lbs.; there is no evidence of anæsthesia.

The patient is greatly emaciated, looks haggard and cachectic, and appears mentally to be somewhat weak-minded. He has a harsh, dry, and non-perspiring skin; the urine is free from albumen and sugar. The sounds of the heart are fairly pronounced and free from murmur.

The case is interesting in the age at which chorea was developed, its causal relation with worry and mental distress, in its being ushered in by weakness in the legs—in which the choreiform movements first showed themselves—and for its extreme chronicity. The disease is so typically one of chorea that it is unnecessary to discuss conditions which might be confounded with it, such as disseminated sclerosis and myoclonus multiplex.

The only point to which I would direct attention is the probable pathological lesion which exists in chorea. No one will refuse to admit the interest which is centred in the inquiry as to what is the pathological physiology of chorea—an inquiry rendered all the more complex, considering the various and inharmonious details recorded

in reference to its morbid anatomy. I do not propose here entering into a discussion regarding the different views as to the spinal or cerebral origin of chorea, or the various experimental observations which have been recorded as to the mechanism of its production, but I shall shortly state the reasons why I consider that the hypothesis which refers chorea to a disturbance in the action of the cells in the motor area of the cortex, is the one which is most in harmony with the origin, nature, and course of the disease.

It will be conceded, however doubtful may be the localisation of the site of the lesion in chorea, that its effects are manifestly those of functional disturbance in the action of the motor nerves.

The ordinary form of the disease is a motor neurosis, which has the following distinguishing features:—It is most frequently developed suddenly in children of good health as a result of emotional disturbances, such as fright or the depressing influences of dread or anxiety; it arises not uncommonly from imitation, or from what is called the *contagiousness* of chorea; it is usually of a transient nature, ending in perfect recovery, and its progress is frequently terminated by the occurrence, during its existence, of some intercurrent disease attended with feverish disturbance. We have next to note the marked predominance of the disease in the earlier stages of life, from the period of the second dentition to the close of puberty, and its special tendency to attack girls. That is, at the period of life when the most active development is taking place as regards the motor function, but when the different lines of resistance to nervous discharge are in a condition of evolution, and the nerve-cells themselves are highly impressionable and unstable, the condition of irregular spasm is set up. It occurs more frequently in girls, from their greater liability to the effects of psychical disturbance. Is it not more likely, from *à priori* considerations, that the origin of this motor neurosis is to be traced to a primary disturbance of the highest motor centres than that the point of departure should be sought for in a lesion of intermediate structures like the basal ganglia, or localised areas in the spinal cord? No doubt marked lesions have been recognised in the corona radiata, corpora striata, and optici thalami on the one

hand, and in the spinal cord on the other, apparently supporting the views of Charcot, Tuckwell, Broadbent, Hughlings Jackson, and Bastian as to the cerebral nature of chorea, and those of Chauveau, Paul Bert, and Onimus as to its spinal origin. But we may fairly ask if the morbid anatomy in the brain and peripheral nervous system, which has been described, is anything more than secondary results occurring in rare and grave cases of the disease as a consequence of a persistent irritative lesion, involving the highest motor nervous mechanism. No doubt the effects upon muscular movements would be the same whether the source of irritation was situated in the corona radiata, in the neighbourhood of the internal capsule, or in the motor area of the cortex, but in favour of the last localisation the following points may be urged:—

(*a.*) The occurrence of an irregular distribution of motor and sensory paresis in chorea.

(*b.*) The greater implication of the upper than of the lower extremities in the choreiform movements—that is, of the limbs most closely related to volition.

(*c.*) The frequent connection of chorea with mental weakness, and the fact that it is attended at times with acute mental disturbance, so as to constitute a special type of the disease under the name of maniacal chorea. Senile chorea is said by Charcot to be usually associated with dementia, and other observers have recorded cases which were indistinguishable mentally from dementia paralytica. One seldom fails to notice a look involving a certain amount of fatuity in most cases of chorea.

(*d.*) Chorea is closely related to hysteria, and the two affections are united in the form recognised as hysterical chorea. Anyone who is familiar with Charcot's description of rhythmic chorea must recognise how largely the hysterical element enters into the genesis of the motor disturbances. This is all the more important to recognise, considering the relation which hysteria has to alteration in the function of the nerve-cells of the cerebral cortex. I believe in time a very close relation will be established as regards the mode of production of motor disturbances in chorea, hysteria, and epi-

lepsy, inasmuch as all three most probably depend upon disturbances—it may be nutritive in character—in the highest part of the nervous organism.

(*e.*) The occurrence of reflex chorea, such as that met with in pregnancy, is capable, I believe, of a more intelligible explanation by referring it to disease in the cortex than to the lower nervous centres.

(*f.*) Lastly, the occurrence of habit chorea, which is a condition most markedly under the influence of the will.

With regard to the nature of the lesion affecting the cerebral cortex, it is difficult to give a satisfactory opinion. One might be disposed to theorise and urge that the occurrence of some sudden disturbance of the circulation, probably brought about by the violent action of a psychical emotion upon the vaso-motor centre in the medulla, affected the circulation through the second and third branches of the middle cerebral arteries. We should not, however, be unmindful of Virchow's teaching, that we are not to look to the vessels for the initiation of disease in the textures of the body, that the life of the texture is in the texture itself, and that the demand for increased or diminished supply of blood made to each texture, and the movement in the blood consequent thereon, is secondary to the demand made by the texture elements themselves. Upon this view we must look to the initiation in chorea being taken by the nerve-cells themselves, and that any changes affecting their nutrition is induced secondarily by alteration in the demand which they make for an increased or diminished blood-supply.

DIVER'S PARALYSIS.

This case, kindly sent to me by Dr. Myles, is one of which I am only able to give incomplete details. I had an opportunity of examining the patient but once in hospital, as, to my disappointment, he left after a sojourn of two days. He was a tall, powerfully developed man, with a tendency to corpulency, and aged thirty-four years. After serving as a sailor for seven years he became a diver. About three months ago he was engaged as a

diver at Trinidad, at the extraordinary depth of 143 feet below the surface. About twenty minutes after emerging from the water he lost power over his legs, and they became numb and dead to all feeling. In a short time some power returned in them as well as sensibility, and he returned to Liverpool a month ago. From the period of the seizure he suffered repeatedly from attacks of vertigo, especially after exertion, the vertigo being preceded by a numb, cold feeling in the occiput, or a sensation as if he had his hat on. He has the same kind of feeling and a sense of tingling in the legs when he sits down on a chair.

When I saw him in hospital I found the condition such as would indicate chronic myelitis in the dorsal region of the cord, with signs of irritation or degeneration in the pyramidal tracts; motor paralysis, some anæsthesia and analgesia of the legs, especially the latter, and partial loss of faradic and galvanic excitability of the muscles. The plantar-reflexes normal, cremasteric absent, exaggeration of patellar and quadriceps reflexes on both sides, but no evidence of ankle clonus. The patient found it very difficult to walk or stand, and on attempting either the legs from the knees were widely separated, so as to simulate a condition of extreme knock-knee. His gait was of a waddling character, such as one sees in a more marked degree in pseudo-hypertrophic muscular paralysis. When standing, after making some exertion, a very marked clonus was spontaneously developed in the quadriceps extensors on both sides; the clonus distinctly affected the vasti as well as the recti muscles. The muscles of each calf were rigidly contracted, and the patient had to take long steps before he could bring the sole of his foot to the ground. When lying in bed both legs were kept in a condition of rigid extension, the feet being in the position of talipes equino-varus, and the toes extended upon the foot. The position noted was that usually seen in primary lateral sclerosis. The foot was so extended as to suggest a partial subluxation of the astragalus, but the patient accounted for the extension of his feet by the fact that he had to use heavy leaden plates, which were strapped to his feet so as to keep him erect when descending through the water. He further remarked that a

swaying kind of gait, such as he presented, with the feet wide apart, was quite common amongst divers, from the constant use of these plates, which weigh 15 lbs. There was no pain complained of anywhere; some irritability of the bladder and an impaired power of retention existed, but not to any marked degree.

The case just imperfectly narrated is an instance of what is known as the caisson disease, produced in persons who, after being subjected to a very great degree of increased atmospheric pressure, are suddenly relieved from this pressure, as in the case of men working in submarine operations by the aid of compressed air. It is stated that no disease is produced except the pressure exceeds 15 lbs. to the square inch, but that its intensity varies directly with the pressure, which sometimes exceeds over 50 lbs. to the square inch, and also with the length of time the pressure has been exercised. Amongst the most common of the lesions produced by the use of the caisson is paraplegia of the lower extremities, and the peculiar features of the disease appear to be as follows :—

It is developed usually half an hour after the diver reaches the surface; it generally involves sensation, motion, the use of the sphincters of the bladder and rectum; its duration usually varies from twelve hours to some weeks (though in a certain proportion of cases the lesion becomes permanent), and the paralytic phenomena reach their maxim of development in the course of a few minutes.

Two views have been urged to account for the lesions observed in the brain and spinal cord in the caisson disease. François and Paul Bert hold that the symptoms are caused by the liberation in the vessels of gases which are absorbed under increased pressure, but set free when the pressure is removed. Paul Bert, in the *Comptes Rendus* for 1872-73, demonstrated the existence after death of bubbles of nitrogen in the vessels of the brain and spinal cord of animals subjected during life to a pressure of air exceeding five atmospheres, and he points out that when pressure is less than five atmospheres, and that three minutes are allowed for the restoration of the normal pressure, the formation of the globules of nitrogen is prevented.

The great objection to the theory of François and Paul Bert is that, admitting the effect of the pressure of over five atmospheres, maintained for a period greater than three or four minutes, to be attended with the absorption of gases, and that these give rise to obstruction in the vessels, it is difficult to explain the immunity which most divers have from any but trivial disturbances—such, for instance, as tinnitus aurium, sensations of pins and needles, &c., although many work for an hour at a time at a depth of from 90 to 100 feet below the surface. The most rational explanation is that which refers the lesions to changes in the circulation resulting from the sudden removal from a condition of increased atmospheric pressure. The effect of the increased pressure on the surface of the body tends to drive the blood towards the internal structures; the equalisation of blood pressure sends an enormously increased supply to the vessels of the brain and spinal cord in such a degree that the distension of those vessels is so great that they are regarded as passive tubes, which are more or less removed from the normal influences which regulate blood supply. When the external pressure is suddenly decreased to the normal standard the blood is at once directed from the channels which have been over-distended. But as the vessels of the brain and spinal cord lie within osseous canals, the diminished pressure on the surface of the body affects them only indirectly. The blood pressure in the cerebral and spinal vessels is suddenly lowered at a time when the vaso-motor mechanism is out of gear, so that, although the vessels are full of blood, the circulation becomes slowed, and areas of stasis may be formed, which, affecting the nerve elements, give rise to symptoms of nervous disturbance—such as pain, paralysis, vertigo, &c.

Why the paralysis is generally confined to the lower limbs is a matter of considerable interest, and I do not think the explanation of it offered by Gowers a satisfactory one. He regards it as due to an acute revulsive anæmia, such as may be developed after severe hæmorrhages; but this does not fully account for the paralysis being a lumbar or dorsal paraplegia.

One would be disposed to regard the symptoms as due to a hæmorrhage into the cord; but in two *post-mortem* examinations

made in paraplegia developed in divers, recorded by Leyden and Schultz, no traces of hæmorrhage were found, the cords showing merely evidence, slight in character, of disseminated sclerosis, principally existing in the dorsal region. I am inclined to regard the explanation offered by Moxon as the true one. He points out that the blood supply to the cord is carried on by three long slender vessels coming from the vertebral arteries, and that these are reinforced by small branches which pass to the cord along the nerve roots. These branches are for the most part short, nearly transverse in direction in the cervical and dorsal regions, oblique in the lumbar and sacral regions. Furthermore, it is to be noted that the spinal cord does not pass beyond the lower body of the first lumbar vertebra, whilst the spinal canal enclosing the leash of nerves, which form the cauda equina, extends to the end of the sacrum. Along the obliquely directed sacral nerves pass upwards those delicate branches of the lower lumbar and sacral arteries which reinforce the terminal portion of the spinal arteries proper. The lumbar enlargement of the cord receives therefrom its supply of blood under conditions of blood pressure, which a slight weakening of the vis à tergo must reduce to a minimum. This is shown by the fact that the anterior spinal artery is rarely found injected to its termination, usually, I believe, to only within a couple of inches of the filum terminale, whilst the vessel is always found empty on the filum itself.

Professor Birmingham, who kindly made an investigation into the arterial supply of the lower part of the cord for me, mentions a very curious point in reference to the length of the reinforcing sacral arteries. That corresponding to the third or fourth sacral nerve was eight inches in length, of very small calibre, and injected only at its lower part from the lateral sacral for a very short distance, whilst the part in immediate connection with the anterior spinal artery was also injected, but the intervening part of the vessel was found empty.

Thus it is seen that the end of the spinal cord, corresponding to the innervation of the lower limbs and the sphincters, receive a supply of blood under conditions which circumstances may render

precarious, for any sudden diminution of blood pressure may lead to such a reduction of blood supply as to produce profound functional disturbance. This may be aggravated by any sudden increase in the passage of the cerebro-spinal fluid within the theca spinalis. What probably helps to avert disturbances of nutrition depending upon defective blood supply in this situation is the greater number of reinforcing arteries at the lower segment of the cord than elsewhere. I believe, then, the peculiarities of the circulation just described account for the paraplegia developed in divers, being usually of a transient nature. It is true the case which I have brought before the Academy represents a dorsal form of paraplegia, and not a lumbar one. But I consider that in this case, and in others recorded of a similar chronic nature, the sudden disturbance in the circulation led to such nutritive changes in the cord that a myelitis was set up in that part of it which is, *par excellence*, the seat of inflammatory change—viz., the dorsal segment.

ON ACUTE ENDO- AND PERICARDITIS.

By MICHAEL F. COX., M.K.Q.C.P.,
Physician to St. Vincent's Hospital.

[Read in the Section of Medicine, April 5, 1889.]

I do not think it can be necessary to apologise to the Academy for introducing the subject of acute endo- and pericarditis; the frequency of the occurrence, and the gravity, of these affections, should be ample excuse. And yet it may be said, or thought, that the number of those—and they of such eminence—who have, in Dublin, treated of these diseases, has been so great, both in the past and in recent times, that it is little, if at all, short of presumption on my part to bring forward a subject with which the names of Graves, and Bellingham, of Stokes, and Hayden, not to mention the living, have been so honourably identified. And yet as Science advances, and as we all think, or *think* we think, independently, even so, with all diffidence, I venture to present my thoughts and observations on the matter before the Academy. I shall not presume, and do not pretend, to deal exhaustively, or perhaps systematically, with the subject; but merely propose to offer, for the consideration of the Academy, the facts which I have observed, and the ideas which have occurred to me, in connection with these facts.

I shall divide my remarks into three heads—first, as to the frequency of acute endo- and acute pericarditis; secondly, as to the causation of these affections; and, thirdly, as to their treatment.

In the first place, I have been greatly impressed by the enormous frequency of these affections, in private as well as in hospital

practice. I cannot suppose that my experience in this respect differs from that of other Members or Fellows of the Academy, but certain it is that I do not meet with any other class of acute affections more frequently than I meet with acute endocarditis. I do not know, as I have said, whether or not the experience of others coincides with mine; but, if the experience be common, we must, I think, conclude that it points to a change in the type, or in the prevalence of a particular type, of disease—such a change as has occurred, for instance, in the case of typhoid and typhus fevers. Certainly, with regard to acute endocarditis, I meet with it much more frequently than it seems to have occurred in the practice of Graves, or Stokes, or Hayden, or than recent authorities record it. The varieties of acute endocarditis and acute pericarditis with which I shall deal are the rheumatic and the idiopathic, as well as with what I prefer to call the septic variety of endocarditis. And here I must confess myself at a loss with regard to the differentiation between so-called idiopathic and rheumatic endocarditis. *Does* such a thing as idiopathic endocarditis exist? I cannot definitely undertake to answer; but certain I am that I have met with a considerable number of cases of acute endocarditis, of a mild type for the most part, and of brief duration and favourable termination, in which neither joint-trouble nor perspiration, nor, if I may so call it, *rheumatic*[a] tongue, was present. I have records of nearly a dozen such cases, as well as of two cases of so-called *ulcerative* or *septic* endocarditis, and of one case of acute idiopathic pericarditis.

Authorities differ widely as to the prevalence of acute rheumatic endo- and pericarditis; the mean estimate being that about 50 per cent. of all first attacks of rheumatic fever are attended by some form of cardiac complication. My experience coincides more closely with that of Dr. Latham, who places the percentage considerably higher. Perhaps the explanation which he offered would also apply to me—namely, "the assiduity of his clinical clerks in providing interesting cases for his wards." As to the relative frequency of endo- and pericarditis, I also find myself more in

[a] White, creamy, furred tongue.

accord with Latham and Sibson, than with Stokes or Hayden, Fuller or Bamberger. Latham found acute endocarditis present in three out of every four cases (75 per cent.) of rheumatic cardiac complications, and acute pericarditis in only one out of twelve; whilst the two affections co-existed in one out of every seven cases. Of Sibson's cases, according to Hayden, four-fifths were of endocarditis, one-tenth of pericarditis, and of the combination of endo- and pericarditis also one-tenth. No account is here taken of myocarditis. Fuller found endocarditis and endo- and pericarditis combined, collectively, in one out of 2·3 cases, and pericarditis alone in one out of every six cases.

Now Stokes and Hayden found acute pericarditis more frequently than acute endocarditis in rheumatic fever, although their experience differed as to the relative frequency of acute pericarditis occurring in combination with acute endocarditis—Stokes finding the dual combination more frequently than any other cardiac complication, and Hayden finding it less frequently than any other: except, perhaps, myocarditis, which did not enter into their calculations; neither having ever met with a case in which it existed independent of endo- or pericarditis. Whilst Stokes barely admitted the possibility of its existence or recognition, Hayden and other good authorities admit its existence. I myself have met with one case of its separate existence, one recently in which it existed with endocarditis, and perhaps one in which it existed in combination with pericarditis.

There are, then, some propositions, or statements, of Hayden and of Stokes which my experience, so far, controverts. The first is that in rheumatic fever pericarditis is more frequent than endocarditis; and that when the latter exists, the former is likewise generally present. Secondly, that the liability to endocarditis is in direct proportion to the severity of the joint affection and to the number of joints engaged. Although Hayden admits that endocarditis *may* exist independent of joint-trouble in the rheumatic, he does *not* admit the existence of acute idiopathic endocarditis. Thirdly, that the liability to cardiac complications is in direct proportion to the severity and duration of the pyrexia.

Fourthly, that (according to Hayden) the murmur of acute rheumatic endocarditis is usually mitral in *origin*, and lasts for life, "usually coeval," he says, "with the life of the patient"—differing herein from Sibson, Latham, and others; although he seems to assent to the statement of Lancereaux that in *ulcerative* endocarditis due to paludal miasma the aortic valves are usually the first engaged.

Now, as I have stated, my experience is directly opposed to that of Hayden and Stokes in these respects. In the first place, I have found—as Flint, Niemeyer, Bamberger, Fuller, Sibson, and Latham found—that endocarditis is much more frequent than pericarditis. Secondly, I have found the liability to endocarditis to be in the inverse, and not in the direct, ratio to the severity of the joint-symptoms.[a] Thirdly, I have not found the liability to cardiac affections to be in direct, but rather in inverse, proportion to the severity and duration of the pyrexia. I have frequently noticed that the heart became engaged when, and as, the temperature fell—as a salt crystallises when fluid cools; and that endocarditis flourishes with a low, even with a subnormal temperature. Fourthly, I have found acute rheumatic endocarditis generally curable, agreeing herein with Latham and Sibson; Latham finding that nearly one-fourth left hospital apparently cured—free, at all events, from murmur; and Sibson finding as many as one-half of his cases recover; whilst Hayden lamented that he had not had the good fortune to meet with more than one case of complete recovery out of some hundreds.

As to the locality of origin of the endocardial mischief, I find the aortic outlet to be generally the first affected; but the mitral outlet, although engaged later, generally the more permanently affected.

In rheumatic endocarditis, I find that the pulse generally gives, by its altered character and tone, the first indication of the onset of the disease; whilst a softened systolic, or, in some cases, a post-

[a] I have rather found the less the joint-trouble, the greater the danger of heart-engagement, and *vice versâ*.

systolic, or diastolic, aortic sound generally precedes, by some days, the invasion of the mitral area.[a]

I submit some charts, which show not alone the possibility, but the actual occurrence, of endocarditis, apart from, and independent of, any elevation of temperature; and these charts are in no way specially selected, but rather types of what I generally find. This *apyrexia* has an important bearing on the question of treatment, for, manifestly, if there be no pyrexia, and no pain, there is no scope for drugs that have the faculty of easing pain or reducing temperature—on the contrary, they may do much harm, by lowering the vital power.

I submit here the charts of four cases of acute rheumatic endocarditis, which I have recently had under my care in St. Vincent's Hospital, in which the disease existed quite independently of any fever; in fact, as will be seen, for the greater part of the time the temperature was, in a marked manner, subnormal; and in one case improvement was coincident with a slight rise in the temperature. This last was the case of a delicate girl, a servant in a country house near Dundrum, which has proved a rather fertile nest for rheumatism, as I have had the wife and three children of the gardener, who live in the yard adjoining, suffering at various times from rheumatic endocarditis, two of them without any *arthritic* trouble. This girl had not much arthritis, but developed very bad endocarditis, which first manifested itself at the aortic outlet, but quickly migrated to the mitral area, and firmly established itself there for some weeks. Under the influence of treatment she gradually improved. She left hospital ten days ago; and I saw her yesterday, when the murmur had completely disappeared. Although she has *no* murmur, she is, as she always has been, very anæmic; and so, when leaving, I put her on full doses of the tinct. of the perchloride of iron, which she is to continue.

The second case which I submit is that of a young constable in the Dublin Metropolitan Police—a body of men who, from constant exposure to wet and cold, are especially subject to rheumatic

[a] The metastatic character of rheumatism is sometimes shown, no less by its jumping, as it were, from one joint to another, than by its *metastasis* from one cardiac area to another.

attacks. This man had recently left another hospital, after having recovered from an attack of rheumatism, and had resumed duty in very severe weather. Although on duty, he was feeling very ill; he had no joint-pains, but complained of malaise and sick stomach. On examining him, I found his pulse quickened and irregular, being occasionally intermittent. His temperature was nearly 101° F. The first sound was softened at the aortic outlet, and the heart's action was laboured. At my suggestion, he was permitted by Dr. Nedley to come into St. Vincent's Hospital. For three weeks I had him confined to bed. The temperature throughout was *subnormal;* the cardiac intermittency lasted for more than a week. At first, as I have said, there was softening of the first sound at the outlet of the aorta; then, in about a week, a murmur became developed at the mitral orifice, and remained for about ten days, when it cleared off; and now there is no trace of it, whilst a very slight alteration of the first sound at the aortic outlet remains. In this case myocarditis existed with endocarditis.

The third case is that of a boy, aged sixteen years, who came into the hospital on the 13th of last month (March), suffering from rheumatic fever. The joints only were engaged; the heart was unaffected. His temperature on admission was 100° F.; after some days it sank to the normal; and *then* endocarditis manifested itself, first revealing itself at the aortic, and then at the mitral outlet; thenceforth the temperature was, for the greater part, *subnormal.*

The fourth of these cases was also that of a constable in the Metropolitan Police, and, like the former case, he had recently left hospital after an attack of rheumatic fever. On admission he was suffering very much joint-pain, especially in his knees, shoulders, and elbows, and perspiring copiously. He had no heart affection. His temperature was 101·5° F. Under the influence of thirty-grain doses of salicylate of soda three times a day, and of blistering, the temperature steadily and quickly fell, and pain was relieved. *Then* endocarditis presented itself in softened cardiac sounds; and although the salicylate of soda was discontinued, the temperature thenceforth remained normal or subnormal.

The next case which I shall present to you was of a very different character. It was the case of a young fellow named Keegan, aged twenty-nine years, an intelligent and sober shop-assistant, who was admitted to hospital on the 10th of October last (1888), with symptoms which resembled typhoid. He looked chilly and pale; had a whitish-brown tongue, and a temperature of 102° F. He had got a wetting a fortnight previously, and felt unwell in the interval, but had no joint-pains. Being struck by the character of his pulse, which was rapid, soft, and yielding, I examined his heart, and found endocarditis already developed. On careful examination in bed, I found a loud-toned, rather musical, presystolic mitral murmur present. He had no joint-trouble. The temperature, as you may see, oscillated violently for some days, rising as high as 103·4° F., and falling as low as 97° F. On the sixth day after admission it suddenly sprang from 97° F. in the morning to nearly 106° F. in the evening, and in a few hours fell again (quickly, almost as mercury) to 97·5° F., subsiding a degree further before next morning, to 96·4° F. The rise in temperature was attended by anxiety and distress, but unattended by cerebral disturbance. The fall was marked by considerable prostration. The Resident Pupil had given, on observing the rise in temperature, twenty or thirty grs. of salicylate of soda, and attributed the fall of temperature to the action of the drug; but I thought it quite inadequate to effect such a profound change; and attributed the rise to profound central nervous disturbance; and the fall to reaction. The variation of temperature, amounting between the rise and the fall to between 18° and 19° F., was most remarkable, and seemed to me of bad omen. The case went on with fluctuating temperature, varying between 97° and 102° F., until it had sunk as low as 96° F., when it began again to climb up by rapid steps until it reached 104° F. on the evening of the 9th November. By next morning, just a month after admission, it had fallen to 97·4° F., a fall of nearly 7 degrees. As I have already mentioned, on admission the patient presented a loud, well-marked presystolic mitral murmur. This murmur now suddenly became altered in character; it got much

lower in tone and less distinct. This apparent improvement did not, I explained, augur well. Taken in conjunction with the rapid rise and fall in temperature, which favoured hyperinosis of the blood, I diagnosed cardiac thrombosis; and prognosed the danger of embolism. For some days the case progressed quietly; when suddenly one night the thrombus became dislodged from its moorings and was wafted into the brain, lodging in the right middle cerebral artery; and next morning I found my patient with left hemiplegia. It happened to be my class morning, and pointing out the case to my pupils—who had carefully watched its progress—I stated what had, in my opinion, occurred—namely, that the thrombus, the formation of which we had recognised, had become dislodged from the mitral orifice; and passing from the heart had been arrested in the right middle cerebral artery; and, if that were so, then I said the recently-altered presystolic mitral murmur would have probably regained its pristine tone. And so we found it. It needed little prophetic power to tell what the result should be—embolism, softening, and death. In a brief space the whole cycle of rheumatic endocarditis was traversed before our eyes. Rapid oscillation of temperature continued, a rise of over 5 degrees being quickly followed by a fall of 6, and then succeeded by rapid zig-zag rise and fall—like lighting flashes—until finally it sprang upwards and was lost in *ante-mortem* hyperpyrexia.

The next case which I will very briefly refer to is that of P. D., formerly a coal porter, and subsequently hospital porter at St. Vincent's; who has had no less than a dozen different attacks of rheumatic fever—characterised by excessive arthritic pain—but never by any cardiac complication—except possibly once, by myocarditis. In one attack his life seemed in imminent danger from hyperpyrexia, attended by restless and violent delirium, both of which were arrested and his life probably saved by severe blistering to the nape of the neck, and a large dose of quinine. I mention this case because of its bearing on the causation of hyperpyrexia, and its treatment.

The next case which I shall record occurred in private practice, and deeply interested me. It was a case of what must be regarded as

septic, typhoid, or ulcerative endocarditis. Its course extended over five weeks and terminated fatally. I saw the case in the beginning once, then twice, frequently three times, and occasionally as often as four times a day. It was seen on consultation repeatedly with me by Dr. Redmond, than whom no one could desire a more loyal or helpful colleague, and was also once seen in consultation with us by Dr. Nixon. It was the case of a young lady, aged twenty-four years, whose father had died, at a comparatively early age, of cerebral embolism, or hæmorrhage; and whose mother had died of broncho-pneumonia degenerating into phthisis. Her brothers and sisters were not robust; one brother was threatened with phthisis, and had taken a voyage to Australia, where he had been advised to remain at least for some years, because of pulmonary delicacy. The girl herself was slight but hardy, and was very fond of exercise. She had spent her holidays yachting around the Scotch coast, but did not seem to derive the benefit which was expected to follow from such a healthful and enjoyable holiday. On the contrary, she felt readily fatigued, and, before I was called in, had been to see a leading member of our profession here in Dublin. She was complaining at the time, in addition to want of strength, of want of appetite and disordered stomach. Feeling so weakly, she or her sister had suggested to the physician whom she consulted, to give iron. I only mention this to show that she merely thought herself out of sorts. However, the doctor, more wisely, finding her stomach disordered, prescribed for her a stomachic mixture; bicarbonate of soda in a bitter infusion. Evidently she had no cardiac affection at that time. Some days afterwards feeling very ill, and seeming feverish, a friend, a senior and well-informed medical student, induced her to take 10 grs. of sulphate of quinine. A few days afterwards, on the 11th of December, 1888, I saw her for the first time. Her temperature then was 103·4° F., her pulse 120. She presented a typhoid aspect; had a coated tongue but no diarrhœa; no eruption and no cough or lung trouble; she had not then, nor had she had previously, nor had she subsequently, any joint pain whatever. I inquired into the condition of the closets and drains but found nothing wrong. The house is in a healthy, elevated,

airy locality, in a southern suburb—a rather new but dry house—on good soil. On examining the heart I found well marked endocarditis present, marked by a prolongation and softening of the first sound at the aortic orifice; the mitral and pulmonary areas were then unaffected. By next day the temperature had fallen to 99° F., but the day following it rose to 104° F.; and later on that day to 105° F.; subsequently falling that same evening to 102·8° F.; next morning it had risen to 103·3° F.; later on it sank to 101·2° F.; at night rose to 104° F.; next morning sank to nearly 100 °F. (100·2° F.); the next day it sank as low as 99·8° F., when menstruation occurred; but remained for only a short time. During the next week the case pursued a varying course, the temperature varying between 100° and 103·8° F. By the 25th of December (Christmas Day), the temperature was normal. For a week all went well. The endocarditis which had originally declared itself at the aortic orifice had shifted, first to the pulmonary, and then to the mitral outlet, where, for a time, it established itself. But the condition was steadily improving and promised a favourable termination—coincident with the fall in temperature, and the general improvement—till the first of January, when a fresh Alpine ascent was, so to speak, begun; and it became evident that a fresh battle had to be fought. By the 3rd of January the temperature had reached 104·4° F., the pulse 120. The respiration was never quickened. Gastro-enteritis of a very acute and severe character now set in, attended by vomiting and diarrhœa. At twelve o'clock, p.m., on the 4th of January, the temperature reached 105° F., but fell next morning to 100·8° F. Then for three days it varied from 102° F. to 104° F.; rising the day following (the 9th), to 104·8° F.; then it fell to 101° F. By this time the patient had become greatly exhausted; she grew weaker and weaker; was first restless, then delirious, then became unconscious. Diarrhœa was continuous, the odour of the most offensive, acrid, and almost intolerable, character. For some days a catheter had to be passed, urine being secreted freely; coma and profound collapse set in; she became algid and seemed moribund. As a last resource I injected a few drachms of sulphuric ether hypodermically. A slight rally took

place—next morning the patient was semi-conscious, then became more conscious, then somewhat hysterical. Next day, January 13th, the temperature stood at 99° F.; she was more conscious in the morning; in the evening was quite conscious, and menstruation set in. Next day menstruation ceased, but the tongue was clean and she was quite bright, although the temperature rose to 101·2° F. That night she was quiet but sleepless, and sleepless during the following day, the temperature falling to 99·8° F. Still no sleep; then the mind began to wander; delirium set in, followed by exhaustion; the pulse grew weak; then complete unconsciousness ensued, and the temperature began to rise—creeping up higher and higher, steadily, until 107° F. was reached—and soon after the end came.[a]

My next, and, I am sure, you will be glad to hear, my last, two cases are of pericarditis, the one with, the other without effusion. With these I will be brief. I shall deal first with the case of pericardial effusion. The patient, a schoolboy, aged fourteen years, was admitted to St. Vincent's Hospital on the 12th of February, 1889. He was a delicate, strumous-looking boy. His temperature on admission was 100·5° F., but it rose that evening to 102·5° F., and afterwards ranged between 99° F. and 102° F. His family history was fairly good. He himself had had scarlatina and typhoid fever, but they left no apparent ill effect. The history of the present illness was, that about a month before admission to hospital he had been thrown and hurt at football. A week afterwards he felt pain in the right side and epigastrium, and consulted a medical man who ordered him a plaster, which relieved the pain for a time. Subsequently another doctor, who was called in, seems to have diagnosed enlargement of the liver; for which he ordered a blister. On admission to hospital the mark of the recent blister was quite fresh. The liver was found pushed downwards, and above the blistered surface was red and puffy as if pus had formed there; but the swelling seemed due to the irritation of the blister. The right side was

[a] I have since had another case of acute ulcerative, septic, or malignant endocarditis, which terminated fatally, in six days, by embolism, hemiplegia, and hyperpyrexia.

found greatly widened and flattened, being a few inches more in girth than the left, and was quite dull on percussion. The breathing was very hurried, and the pulse small, wiry, and rapid. Pleural effusion, displacing the liver downwards, was diagnosed, and verified by aspiration, which brought away nearly a quart of serum. A slight friction sound was audible at the summit of the dulness, but was barely perceptible before operation; however, a loud friction sound developed after the removal of the fluid, and thenceforward the pleuritis quickly cleared off without any recurrence of the fluid.

At the same time, however, as the pleural effusion was diagnosed, it was found that there was also pericardial effusion. The heart sounds were scarcely audible, and dulness was found to exist from the top of the sternum to the ensiform cartilage, and from the right of the sternum for some inches transversely. Dulness also extended from below the left nipple to about the third rib on the left side. I had almost determined to have the pericardium tapped; but I resolved to give a chance, poor though it seemed, for absorption. Accordingly I ordered the patient 1 gr. of grey powder with 5 grs. of saccharated carbonate of iron three times a day, and had the præcordial region painted with lin. iodi. After some days I was gratified to find an apparent shrinkage in the extent of the effusion, and then, after a few days more, a friction sound appeared toward the apex of the heart, which gradually crept upwards until it was audible all over the cardiac region, then it began to fade from below upwards until finally and quickly it completely disappeared—and with it the dulness vanished; the boy gradually improved, lost his dyspnœa, and gained strength; syrup of iodide of iron with cod liver oil was substituted for the hyd. c̄ creta and fer. carb. sacch., and the patient steadily and satisfactorily progressed towards apparently complete recovery. He has now left hospital with no effusion, and, so far as one can judge, with no adhesions.

The last case—positively the last—is that of a gentleman who happens to be a member of the Irish parliamentary party, and will not, therefore, I hope be debarred from obtaining your sympathy. Just a little more than two years ago, he became suddenly ill in

London. The weather at the time was intensely cold; the House of Commons intensely warm, stuffy and contaminated with sewer-gas. Sittings were late, and leaving the hot, if not *heated*, air of the House of Commons, and going out into the ice-cold, foggy night or morning air of London, with the vital power necessarily lowered by the vitiated air of the House—what wonder that one should, so to speak, catch cold? This gentleman, about thirty-five years of age, was very strong, had never previously, one might say, been ill—since when a child he had had scarlatina. One day he was suddenly seized with violent pain, apparently in the stomach. A doctor was called in, who seems to have prescribed a stomachic. The pain continued, so another doctor, a friend, was called in. He ascribed the pain to *angina pectoris*. During the week or ten days which followed, the patient suffered great pain of a stabbing character; was unable to walk without extreme distress, and had no appetite. He remained up till the Monday week after his attack, for nine days, and continued going about, although with difficulty. He made two short railway journeys on the Monday. Thenceforth he was in and out of bed for three or four days. At this time his medical attendant wrote to me—"I find a very distinct diastolic murmur at the base of the heart, immediately over the painful part, heard more distinctly towards the lower end of the sternum, with muffling of the second sound. I believe the aorta to be the seat of the trouble; but, of course, I cannot tell whether or not it is quite recent." Evidently the case was regarded as one of aortic regurgitation. I crossed over to see the patient, and found him in the early morning on my arrival, with a temperature of 101° F., and suffering from well-marked acute pericarditis. The case was complicated by much gastric disturbance, a common phenomenon in such cases which often proves a source of error. The temperature ran up to 103° F. for the next few evenings. There was great exhaustion consequent on the previous exertions, as well as caused by the fever and sickness of stomach. Collapse was a few times threatened, and at night restless delirium was present. Rectal medication had to be resorted to. Suppositories with quinine and morphia being administered; as well as free

vesication, over the region of the pericardium affected, being resorted to. After a few nights a regular crisis set in, marked by fall in temperature, copious perspiration, and cessation of delirium; and thenceforth the patient made a rapid and complete recovery; and has since, now two years ago, been free from any cardiac trouble; presenting not the least trace of his dangerous malady, notwithstanding much exposure and, perhaps I may say, occasional hardship.

Now a few words briefly as to causation and treatment: To treat of endo- or pericarditis and leave rheumatism out of consideration, were to play Hamlet without the Prince of Denmark. If rheumatic fever cannot say, like Coriolanus, "Alone I did it"—it may at least say of the complications, "*Quarum pars magna fui.*" They are, in truth, well nigh, if not quite, inseparable. The causation and treatment of rheumatism are, therefore, largely the causation and treatment of endo- and pericarditis; but not wholly; for what might, for instance, benefit one aspect of rheumatism might injure another—what relieves the pain or reduces the temperature, may not equally benefit, nay, may interfere injuriously with the heart affection. And yet there is much truth in Watson's saying, that to all intents and purposes the heart must be regarded as a joint in rheumatic fever. Yes, and an important joint, nay, the most important of all, on which all the rest hinge. And yet, although rheumatic fever involves the heart as well as the joints, all endocarditis, and all pericarditis, is not rheumatic: no more than all arthritis is rheumatic. What is the origin of rheumatic endo- and pericarditis is involved in the still larger question—What is the cause of rheumatic fever? Is it a blood disease, or is it a neurosis? Where are its "*fons et origo?*" In the blood, or in the nervous system? I believe it to be a neurosis. All recent investigations, as well as a careful observation of the facts, point in that direction. The researches of the two Mitchells, father and son; of Gull, of Charcot, of Latham; the indications of pathology and of physiology, which is the elder sister of pathology, point alike to its nervous origin. In no other way, by no other theory, can we account for all the phenomena witnessed—for the sudden metastases, for the hyperpyrexia. Lactic acid we must regard as a *product* of the disease,

and not as the disease itself. In the nervous system, whether in the medulla, or in the floor of the fourth ventricle, must we look for the exciting and controlling power. In the dominant trophic centre and in the thermo-taxic, or heat-regulating, centre, lying side by side in the medulla, must we seek for an explanation of the development and, of the arrest, of the changes which take place in rheumatic fever. In them must we seek, and in them only can we find, an explanation of the remedial effects of treatment, be it counter-irritant, sedative, antipyretic, or revulsive. In the thermo-taxic centre, which some place in the medulla oblongata, some in the pons varolii, and some in the floor of the fourth ventricle, do we find a sufficient and rational explanation of the phenomenon of hyperpyrexia—for simple pyrexia stands on a different basis. And in its existence we can see an explanation of the apparent arrest of hyperpyrexia by the employment of blistering.

That purely spinal injuries can cause hyperpyrexia is well-known. Some years ago I saw a case of a labourer who had fallen off a high cartload of hay. A few hours after the injury he had a temperature of 107°–108° F., with spinal paralysis and priapism, which persisted till a speedy death released him from his sufferings.

"Is there," asks Dr. Buzzard, "something which we may call, provisionally, a trophic centre for the osseous and articulary system in the immediate neighbourhood of the vagi? As I have suggested," he adds, "on a previous occasion, the discovery of such a centre would materially help us to explain the remarkable association of cardiac complications with the joint affection of acute rheumatism, as well as the sweating[a]—characteristic of this disease—and the occasional hyperpyrexia which occurs in it. And it might also help to throw light upon the obscure pathology of arthritis deformans."

In this connection, it is interesting to observe that Sir. Dyce Duckworth holds that gout is also a primary neurosis, having its seat in the medulla. Paget also believes in the nervous origin of gout—"There are reasons enough for thinking," he writes, "that changes in the nervous centre determine the locality of each

[a] Due to paralysis of the sympathetic.

gouty process; while changes in the blood and tissues determine its methods and effects; and that thus we may explain the symmetries of disease in gout—sometimes bilateral, sometimes antero-posterior—and thus its metastasis."

It is now admitted that vaso-motor centres exist along the spine, each presiding over a particular vascular area: that each one may be excited reflexly; but that all are controlled by a dominant head-centre, so to speak, in the medulla. This dominant vaso-motor head-centre it is which controls the nutrition of joints, and thus is explained why an injured joint is especially liable to rheumatic attacks—its centre having been excited, fatigued, and partially paralysed; and hence it is that blistering a joint by constringing the vessels, lessens the blood supply and reflexly stimulates the jaded centre. These trophic centres are like so many roots—with the chief centre as a tap root—whose branches, as it were, spread far and wide throughout the body, to heart and joints; and hence are so liable to be influenced by changes of temperature. If a person, weakened from any cause, be exposed to wet and cold—be, in fact, subjected to chill—glycocine, which is formed in excess, is transformed into uric acid; which, acting on the vaso-motor centres, at first stimulates, and then exhausts them; the superficial blood-vessels become dilated and partially paralysed; destructive metabolism is promoted in the muscles, and so lactic acid is produced, which dilating the smaller vessels, stimulates the centres; the paralysis of the sympathetic produced causes perspiration, which aids the kidneys in eliminating the diseased products—uric and lactic acid.

As to treatment I shall be very brief. We have seen how blisters may act beneficially; that they do so I am thoroughly satisfied by experience. Now-a-days salicin, salicylic acid, and the salicylates have taken the place of the once much-lauded alkalies. Of these I prefer the salicylate of soda. That it relieves pain and lowers the temperature, in most cases, I am satisfied—but there its utility, I think, ends. Whilst it is said to prevent the development of cardiac complications, as was claimed for the alkalies, even Dr. Latham, who praises it highly—showing how it breaks up, and prevents the

formation of, uric acid—does not claim that it cures them. It sometimes causes distressing tinnitus, like quinia. Harm is done certainly, if not directly at least indirectly, by the speedy immunity from pain, which is taken to mean recovery; and so patients go forth gladly, to return sorrowfully, labouring under a relapse; it may be suffering from firmly-established endocarditis. In rest, long-continued and persistent, I believe as the sheet-anchor of acute endo- and pericarditis. In repeated blistering, and in the internal and persevering use of iodide of potassium and red iodide, or perchloride, of mercury, followed by the tincture of the perchloride of iron, or ferrum redactum; and occasionally in the employment of oil of turpentine, have I found the only means of mitigating or of curing—if we can cure—acute endo- and acute pericarditis.

TRICHOMYCOSIS NODOSA: A BACILLARY DISEASE OF HAIR.

By ROBERT GLASGOW PATTESON, M.B., &c.

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To many observers it has long been a familiar fact that the hairs of the armpits and scrotum are liable to a peculiar affection which is not known to affect the hairs of any other part of the body, and which seems to find only here those conditions on which its development depends. Twenty years ago Paxton drew attention to the affection in an article entitled—"On a Diseased Condition of the Hairs of the Axilla, probably of Parasitic Origin." His description is full, and, as far as it goes, accurate; but little attention seems to have been attracted until Sir Erasmus Wilson noticed it in his lectures on dermatology, and gave it the name of "lepothrix," regarding its most prominent characteristic as "a loosening and partial detachment of the epidermal scales of the cuticle," which are afterwards "roughened with earthy and saline crusts deposited by the sweat." Continental authors overlooked the condition for many years, and by many it came to be regarded as identical with trichorrhexis nodosa, an error into which some English writers have also fallen. Michelson gives a short account of it, but confounds it with a totally different condition described by Devergie as trichoptilosis. The fullest account is that given by Dr. Radcliffe Crocker in his recent work on "Diseases of the Skin."

Although the descriptions of these observers differ slightly from one another, yet in all essential points they agree, and I have no doubt that the condition is in all cases the same, modified, it may be occasionally, by certain accidental circumstances of environment, and by the health and habits of the individual. Paxton thought he had found the cause of the disease in the presence of spores

and a mycelium similar to those of trichophyton tonsurans. Eberth, in 1874, when engaged in the study of various forms of bacteria occurring in sweat, found in this secretion numerous granular bodies, which bore a proportional relation to the hairiness of the part, and were most numerous in the armpits, and between the thighs. They caused knotty swellings on the hairs, visible to the naked eye, and these he regarded as colonies of micrococci. In a more recent paper in Virchow's Archiv, Behrend, in discussing diseases of hair, redescribes a similar condition due to micrococci, without apparently being aware of Eberth's observations. But these cases which I have had the opportunity of examining have always been associated with the presence of bacilli, the colonies of which lying between the fibres of the superficial cortical layers, and enveloping themselves in a firm homogeneous capsule, cause respectively the splitting of the fibres and the nodes and irregularities of the surface, which are so characteristic of the disease. So far as I can find, after diligent search through the literature of the subject, no description has hitherto been given of a bacillary factor in the etiology of hair diseases.

To the naked eye the hairs present an abnormally dry and dull appearance, and feel rough and knotted to the touch. Instead of presenting a smooth and lustrous surface they look as if steamed. In fact the difference to sight and touch between the healthy and diseased hair is the difference between polished and ground glass. On examining the hair against the light, or better, with a simple lens, the roughness is seen to be due to the projection from the sides of the shaft of numerous very minute concretions, which may assume either of two forms—a diffuse or a nodular. The diffuse form may extend along the shaft for nearly its whole length, with only narrow intervals of healthy hair, while the nodular form consists of small rounded masses, generally most thickly placed on the outer third of the hair, with longer intervening healthy intervals. Both forms not uncommonly co-exist on a single hair, the different appearance being only a question of the degree to which the disease has advanced. These masses are firmly adherent, cannot be removed by forcible rubbing, and can

only be scraped off with great difficulty and injury to the hair. The hairs are not, as a rule, abnormally brittle, do not give way on being pulled, have little tendency to break at these nodes, and generally end in fine lanugo-like points beyond the limits of the disease. Occasionally, however, hairs are met with in which fracture has occurred through a node, and then the terminal point, with the splitting and separation of the fibres, looks, under a low power of the microscope, like a bundle of bristles stuck into a ball of jelly. The disease may affect any part of the hair-shaft, but the "seat of election" is the middle third. Nodes are seldom found well developed except on the peripheral part of the hair, and when the disease travels high up the shaft towards the bulb it usually assumes the diffuse form. Under no circumstances does it ever penetrate into the hair follicle; and in scrapings from the skin of the axilla and scrotum no bacteria were found in the epidermal scales.

The progressive character of the affection from the peripheral end of the shaft to its exit from the follicle, is sufficiently accounted for by the natural history of the disease. The bacilli, journeying as it were from the outer world, travel up the hair until they find a part suited to supply them with the necessaries of life. Here they begin their attack, and here the nodes are closely aggregated, sometimes completely obscuring the shaft, but never losing their nodular character. From about the junction of the outer and middle thirds the nodes are separated by longer intervals, or in more advanced cases the disease may here assume the diffuse form, gradually disappearing as the hair follicle is approached. In those parts which have been last attacked the first stages of their development can be well seen. I have examined a very large number of hairs, and in few was any marked deviation from this typical arrangement to be found; the extremity being always the most diseased part of the shaft. In scrapings from the skin of the part no bacilli could be detected in the epidermal scales. This agrees with an observation of Dr. Crocker, who, in one of his cases, excised a portion of skin, and found the hair follicles and their appendages perfectly healthy. We may, therefore, I think, con-

clude that the micro-organism is one always derived from external sources, and never, under any circumstances, making its habitat in the hair follicles or the sebaceous glands. The mode of formation of the nodes is interesting. The bacilli lie altogether in the cortex of the shaft, and do not penetrate deeply into the substance of the hair. The maceration caused by the moist warmth of those parts, where alone the affection is found, loosens the epidermal scales, and allows an entrance to be effected underneath. This is well seen in those parts where the affection is in an early stage, where the lifting of the imbricated scales, and entrance of the organism beneath, can be easily demonstrated. The bacilli burrow into the hair, and as they develop, the presence of their growth causes a longitudinal splitting of the cortical fibres, which finally tends to their rupture, and the curling-up of the separated ends. At the same time takes place the formation of the hard, homogeneous, or slightly granular substance which forms the nodes, and thus encapsules the bacteria; and into this mass the up-curled ends of the ruptured fibres can be seen projecting. (This may be seen in the unstained specimen exhibited.) These outlying masses also act as splints, supporting the hair at the weakened points, and thus explaining the slight tendency there is to fracture of the hair. The material which is thus thrown out is exactly similar in appearance to that with which the pediculi fasten their ova to the hairs. It is extremely hard, is insoluble in such solvents as alcohol, ether, chloroform, benzol, and spirits of turpentine, and resists the action of strong acid and alkaline solutions. It is in the natural state of a pale yellowish colour, and stains a dark brown with iodine, and a deep purple with methyl-violet. On account of this affinity for the latter dye it is, as a rule, only possible to see the bacilli on the outer edges of the nodes, or in those places where they are just effecting an entrance into the cortex, as the treatment which is required to decolourise the nodes also decolourises the bacilli. The nodular form I have only occasionally observed on the hairs of the scrotum, and then in a slight degree. Very often in this situation few or none of the nodular outlying masses are formed, but the whole extent of the

shaft is burrowed superficially by bacteria, giving the hair a peculiar appearance that reminds one of worm-eaten wood. In other cases the concretions are placed laterally, with little or no separating healthy hair, resembling, as Dr. Crocker suggests, "the feather-end of an arrow."

The bacilli are short fine rods with slightly-rounded ends, about two to three times as long as broad, and about one-fourth the diameter of a red blood corpuscle. They stain readily with any of the aniline dyes, but are best seen when stained by Gram's method. Examined in a hanging drop they are found to be non-motile, exhibiting only Brownian or molecular movement. They are sometimes joined in twos or threes, but seldom form longer threads, though this may be accounted for by the forcible manipulation required to isolate them from the substance which encapsules them. Dr. Purser kindly supplied me with the materials for making some cultivation experiments. Attempts were made to grow the bacilli both on sterilised potatoes and in nutrient gelatine; but both yielded negative results. Indeed it is possible that the bacilli find only in the armpits and scrotum those conditions of warmth and moisture necessary for their development; but this point and their presence in the sweat I hope in future cases to have the opportunity of examining.

Dr. Walter Smith has since kindly afforded me the opportunity of examining some hairs from the axilla presenting the same condition, and in these the presence of the same bacilli was easily demonstrated by Gram's method.

In conclusion, it is only necessary to add that I have ventured to discard the name "lepothrix," conveying, as it does, a false conception of the pathological condition present, and to substitute that of "trichomycosis nodosa," the first term to indicate the bacterial origin of the disease, and the second to indicate its most prominent characteristic, and the one which when present usually leads to its recognition.

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DESCRIPTION OF PLATE.

FIG. 1.

Drawing of a node deeply stained with gentian-violet, showing the bacilli at its peripheral part. At lower part of figure a group of bacilli may be seen in the act of effecting an entrance beneath a loosened epidermal scale.

Leitz $\frac{1}{12}$ hom. im., oc. A.

FIG. 2.

A few separated and outlying bacilli, placed quite superficially at a little distance from the margin of the small node.

Leitz $\frac{1}{12}$ hom. im., oc. C.

Fig 1 × 700

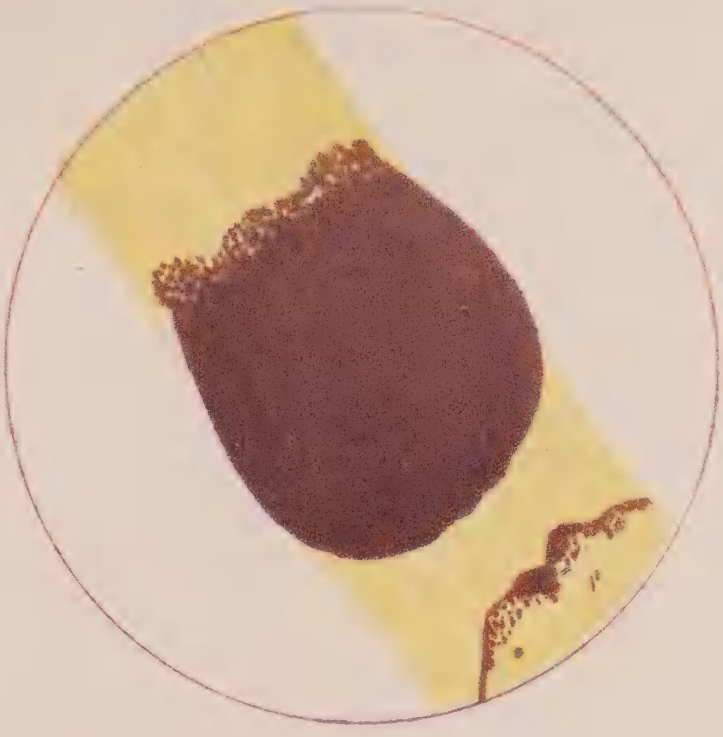

Fig. 2 × 1300

Trichomycosis. Nodosa

[Gram's Method.]

ABDOMINAL SECTION IN TYPHOID FEVER.

By JAMES H. NICHOLAS,
Surgeon, Medical Staff.

[Read in the Section of Medicine, May 17, 1889.]

I HAVE the honour to bring to your notice to-night two cases of perforation of the ileum in typhoid fever; and I shall be glad to have the opinion of those who have so much greater experience than I have as to whether abdominal section would have been justifiable in these cases, or might be carried out in any future case similar to them. The first case is that of a man (Davis) who was admitted into hospital on the 25th November, 1888, suffering from what proved to be enteric fever. He had been feeling ill for four days previously, and it was concluded that the date of his admission was probably about the fifth day of the disease. Spots showed themselves on the 30th November (tenth day of disease), and the fever ran a mild and what appeared to be a most favourable course. The temperature fell to normal on the morning of December 10th, or twentieth day of disease, according to the above calculation, and in the evening rose to 99·6°, leading us to hope for a further fall the next morning. But on the 21st December the man complained of cramps in the abdomen, and the temperature, instead of falling, rose to 102·6°. There was little pain on pressure, and the patient did not appear particularly uneasy. He was, however, immediately placed on opium. In the evening there was a fall of one degree, but the pain in the abdomen had somewhat increased. The next morning there was evidence of extensive peritonitis—anxious face, knees drawn up, and extreme tenderness diffused over the whole abdominal region, which was very tense, besides which were excessive vomiting, cold extremities, feeble pulse, and symp-

toms of collapse. Perforation was diagnosed. The man died at six p.m., eight hours after.

The autopsy showed the abdomen to contain two pints of fæcal matter, all of which was not at first in the cavity, but which poured out from the bowels, as the rest was removed. The parietal layer of peritoneum was healthy, but was slightly attached to the visceral layer, which was intensely congested and inflamed by beads of lymph; the coils of intestine together also by freshly deposited lymph, but were easily separable. The interior of the gut was healthy, except the ileum, and that contained several ulcerations of Peyer's patches, through one of which was a perforation about nine inches from ileo-cæcal valve.

Case 2 is that of a man (Leppard) admitted into hospital, 1888, for enteric fever, and was quite convalescent on January 2nd, 1889.

On January 13th he complained of vague pains in the abdomen, which was slightly tympanitic. There was no sickness and no tenderness on pressure. He was given an opium mixture. On the 16th he had apparently quite recovered, and the pains had left him. On the 20th he was seized during the night with intense pain in the abdomen, and vomited a black, thick fluid; intense pain over the whole abdominal region; great tympany, and the abdomen bulged prominently forwards; pulse full and bounding. He was placed on opium. On the 21st January, the vomiting continuing, the opium was discontinued by the mouth and administered per anum, and he was brought under its influence. There was then little pain, but he was pulseless, with cold extremities, but withal in good spirits.

In the evening his breathing became very rapid, and later he became indifferent to his surroundings (not unconscious), and died at midnight, perforation having been diagnosed fifteen hours previously.

Post-mortem appearances were:—The great omentum was attached by easily separable adhesions to intestine, and the lower end of the ileum was loosely fixed in the pelvic cavity, and covered with long strips of coagulated lymph. There was a half pint of

fæcal matter in a semi-solid condition in the cavity. The peritonitic condition of the small intestine in the pelvis was very marked, and a perforation existed in the anterior wall, 24 inches from ileo-cæcal valve.

In these cases death appears to have resulted from the peritonitis set up by the presence of fæcal matter, and the subsequent absorption of fæcal poison, particularly shown in the latter case by the hurried respiration; and though a certain amount of localised peritonitis was present before perforation, it seems as if the rapid spread were not due to the extension of this area on the peritoneal surface of the gut corresponding to the internal ulceration, but was actually set up by the presence of the fæcal matter.

Similarly, the collapse does not seem to have been the result of the perforation itself, but of the subsequent absorption of the fæcal poison.

Assuming this explanation to be correct, the question arose as to whether it would be justifiable to open the abdomen in any future case that might occur, and, after washing out, sew the opening to the anterior abdominal wall, or excise the ulcer altogether.

The several objections which were urged against this proceeding were:—

1. Difficulty of diagnosis.
2. Condition of the patient.
3. Difficulty of afterwards finding the perforation.
4. Likelihood of causing a second perforation.
5. Diseased condition of walls of gut.
6. That many cases of perforation have recovered.

I have endeavoured to answer these objections thus:—

1. That there are some cases of unmistakable perforation; but if the above statement be correct (*i.e.*, that the extensive peritonitis is the result of fæcal irritation resulting from perforation), it might be deemed advisable to operate in even doubtful cases.

2. As to the condition of the patient, it is evident that if a perforation be diagnosed, and consequently the presence of fæcal matter in the abdominal cavity, recovery is absolutely impossible;

and therefore, though the patient be unfitted for an operation after a most debilitating disease, still, as being the only hope of life, theory would suggest its being performed.

3. Bristowe states that the locality of the perforation is almost always within 24 inches of the ileo-cæcal valve, on the anterior wall of ileum.

4. With ordinary care, it does not seem likely to happen that handling the bowel during the operation would cause a second perforation.

6. Though some cases of recovery be recorded, I submit that, except in those cases where abscess has afterwards formed, there has been insufficient evidence to support the diagnosis; though, doubtless, there may be cases where, the process of ulceration being slow and perforation minute, time might have been allowed for lymph to be deposited around and over the perforation, and so prevent exit of fæces into abdominal cavity.

CASE OF CHOLECYSTOTOMY.

By WALTER G. SMITH, M.D.;
Physician to Sir P. Dun's Hospital;
AND
CHARLES B. BALL, M.D.;
Surgeon to Sir P. Dun's Hospital.

[Read in the Section of Medicine, May 17, 1889.]

Upon the 27th of February, 1889, Dr. Walter Smith was asked to see Mrs. R——, with Dr. Kough, of Monkstown. The lady was on a visit to her mother, and had been in fair health until a few days previously.

On the evening of Monday, February 25th, Dr. Kough was summoned to the patient, on account of violent pain which had attacked her across the upper part of the abdomen. This was apparently due to a fit of indigestion, following a hurried meal of cold meat.

Dr. Kough prescribed accordingly, and next day he heard that she was better, but required an aperient, and he ordered a dose of castor-oil.

He was, however, again sent for on Wednesday morning, and found her looking very ill and anxious, and suffering from intense abdominal pain. The pulse was quiet, and she was free from pyrexia.

Examining the abdomen, Dr. Kough detected a small tumour below the right ribs, and in the afternoon of that day Dr. Smith visited the lady with him.

Her condition then was as follows:—She was cheerful and bright, but in evident suffering. The abdomen was not distended, and readily lent itself to examination, there being no rigidity and no accumulation of fat. The only hindrance was a patch of extreme tenderness between the umbilicus and the ribs on the right side.

Here a slight localised projection was visible, and by gentle palpation a tumour could easily be felt.

The tumour was rounded, directed vertically downwards for about 2½ inches, tense, and excessively tender. It shifted up and down distinctly with the respiratory movements, and could be swayed from side to side beneath the abdominal parietes. The surface was smooth and free from inequalities in form or consistence, and fluctuation could not be made out. There was no jaundice, and the motions were of natural appearance. The temperature was normal.

After due consideration it was thought probable that the tumour was an inflamed and distended gall-bladder, caused by obstruction of the cystic duct, possibly by gall-stones.

This view was based chiefly upon the situation, cylindrical form, projection forwards, and mobility of the tumour, and was strengthened by the sex of the patient and by the reflection that it was difficult to account otherwise for the phenomena. Seeing that the patient had become rapidly worse, and in the face of the probability, if not certainty, of disastrous consequences ensuing upon delay or expectant treatment, the propriety of an exploratory incision was suggested, and a surgical consultation was proposed.

Dr. C. Ball saw her next day with us, and agreed that it was a suitable case in which to operate.

The lady's husband was telegraphed for, and the operation was performed on Friday, March 1st, by Dr. Ball.

As soon as the patient was under the influence of ether the details of the tumour were more fully made out; it extended from the cartilage of the 10th rib to about ¾ of an inch to the left of and below the umbilicus; it was pear-shaped, and had a considerable amount of lateral motion, but was attached above so that it could be swayed backwards and forwards like a pendulum. An incision 1½ inches long was made in the linea alba above the umbilicus, and the tumour was explored with the finger; it was found to be a distended gall-bladder with recent adhesions to the surrounding structures.

The fundus was with a little difficulty brought into the wound; it was deeply congested; flakes of lymph were adherent to it, and

the omentum was attached to it by soft recent adhesions. The omentum was detached, and the peritoneum packed round the tumour with flat sponges; a hydrocele trochar was now introduced and exit given to about 6 oz. of whitish viscid fluid devoid of bile. The opening was enlarged, and a finger introduced, which passed along the dilated cystic duct, but was unable to appreciate the cause of the obstruction. 7 small gall-stones were found free in the cavity, and also a quantity of white putty-like material. The cavity of the gall-bladder was thoroughly cleansed, and it was then seen that the mucous membrane was greatly inflamed, being of a deep maroon colour. The wound in the gall-bladder was carefully sutured to the lower part of the incision, and the upper portion closed by deep sutures. A large-sized flanged drainage tube was left in the gall-bladder, and dry dressings applied. March 2nd—There has only been a little oozing of bloody serum from the wound, but rather copious bilious vomiting. Drainage tube removed. March 3rd—Bile is being discharged copiously from the wound. March 4th—Still discharging bile. A careful search with probe and scoop failed to detect a calculus, but a plug as big as the top of the little finger was removed, formed apparently of inspissated mucus. March 5th—Discharge of bile almost ceased. From this date the wound rapidly healed, and by the 15th day the opening in the gall-bladder was completely closed and the wound soundly united. Since then, however, upon two occasions small quantities of clear fluid have been discharged from the wound. On the last occasion, the tenth day after operation, the fluid was slightly tinged with bile, and since then the wound has remained perfectly healed, and no tumour is to be felt, the patient being in all respects perfectly well.

May 8th—A very good report was received from her mother. The patient was steadily gaining strength, and was free from pain.

P.S.—The putty-like contents of the gall-bladder, when examined under the microscope, exhibited innumerable crystals of mono-calcic phosphate. No crystals of cholesterin were found. The gall-stones were composed chiefly of cholesterin.

SECTION OF SURGERY.

CASE OF PULSATING TUMOUR OF BONE.

By the late ROBERT M'DONNELL, M.D., F.R.S.;
Surgeon to Steevens' Hospital.

[Read in the Section of Surgery, November 9, 1888.]

"There are few subjects in Surgery," says Dr. Richet, the distinguished surgeon of La Pitié, in the opening sentence of his classical memoir on the subject, "on which there is less agreement than on the tumours described under the names—aneurysm of bone, fungating tumours of bone, or pulsating or erectile tumours of the osseous tissue." This he attributes to several causes—first, the rarity of the affection, then the small number of published observations, but chiefly to the difficulty, almost the impossibility, of assigning their true value to clinical symptoms, so that pathological anatomy has been called on single-handed to decide the question. He then adds: "The deductions drawn from clinical observation, a little too much neglected in the present day, have in my eyes a great importance, although in a somewhat different direction from those derived solely from anatomical examination."

The case which I am about to detail owes its value entirely to its clinical history, progress, and termination. My experience of similar cases accompanied with pulsation led me to believe that it was a malignant affection, and I do not hesitate to say that I at one time thought that the only hope of saving the patient's life lay in amputation. Pulsation, however, is a symptom common to several distinct forms of tumours connected with bone, and although, in general, when we have to do with a pulsating tumour of bone, we may feel that in all probability it is a case of sarcoma or very

vascular carcinoma we have before us, yet a considerable number of recorded cases go to show that this opinion must not be too absolute. Some authors, as Lebert and Rokitansky, regarded these pulsating tumours of bone as, in fact, malignant tumours thus modified, and some of those present who have enjoyed the privilege of having been pupils of our eminent surgical pathologist, Professor R. W. Smith, will doubtless remember how decidedly in his lectures he inclined to this view; yet some observations recorded even by the older surgeons—as Scarpa, Pearson, Carnochan, Parisot, Richet, Roux, &c.—left little doubt that there exists a special vascular tumour not essentially malignant, and capable of cure.

Mrs. X., aged thirty-eight years, first came under my observation on the 21st of February, 1885. She brought me a letter from Mr. Erichsen, whom she had consulted a few days before in London. In this letter Mr. Erichsen says—"Mrs. X. has this day (February 18th, 1885) consulted me respecting a swelling in her right leg. I find she has a pulsating tumour, with a loud bruit occupying the middle, outer, and anterior part of the limb. It appears to me to be connected with the fibula rather than the tibia, and the most active pulsation and loudest bruit are about the middle, just opposite that bone. If I were to hazard an opinion as to its nature, I would say that it is a pulsating sarcoma. I have advised Mrs. X. to place herself in your hands, and have told her husband of the gravity of her case."

Mrs. X., thirty-eight years of age, is a healthy-looking, active lady, the mother of three children, and at present nearly four months pregnant. She gives the following account of her case:—Some three years before (in 1882), while attending some meetings in England with her husband, and standing and walking about a good deal, she was suddenly attacked with a severe pain in the right leg, above the outer ankle. This pain was as bad as any toothache she ever felt, and was confined to one spot, on which she could put her finger. She thought a boil or abscess was forming, and remained in bed or in the recumbent posture for some days. In a few days this pain subsided, and since then she has had no acute pain at all. About a year after this she first observed the

throbbing or pulsation in the swelling. She suffered little or no pain or inconvenience in walking, and would not of herself have considered it necessary to consult anyone, had not a friend with whom she was staying in London, and to whom she spoke of the throbbing in her leg, insisted on taking her to see Mr. Erichsen.

The case being one of great interest to me, I made accurate measurements, but I regret that I had not a cast taken of it. The tumour, the position of which is indicated in Mr. Erichsen's letter, to my mind certainly was connected with the fibula; it was about the size of an elongated turkey egg, and pulsated as strongly as any popliteal aneurysm I have ever felt. Pressure on the femoral artery put a complete stop to both pulsation and bruit; at the same time the tension of the tumour was lessened, but even under firm pressure it was not much diminished in bulk, increasing again in three or four distinct throbs when all pressure was removed. Although both the pulsation and bruit were most marked at the middle of the swelling, yet both could be perceived at every part of it.

Grasping the limb firmly in both hands, and squeezing the whole circumference for some minutes, caused no pain, and did not very notably diminish the bulk of the mass. The skin was not discoloured; no vessels could be seen ramifying over its surface. No distinct arterial branches could be found. There seemed, at the upper and lower extremity of the fusiform tumour, to be something like a slight ridge or bony projection; but this was obscure, and it is certain that nowhere over the surface could any evidence of a bony shell be found, or any parchment-like crackle.

I frankly confess that I took a gloomy view of Mrs. X.'s case. I looked on it as a pulsating periosteal sarcoma. As the patient was pregnant, not suffering pain, and in a state of good general health, I advised the use of a moderately tight elastic stocking, rest, and observation.

I saw the patient at intervals of about a month, and finding, on careful measurement, little if any increase, I wrote after the lapse of three months to Mr. Erichsen to report progress, saying that it seemed to me that the fact of pregnancy was assisting the pressure

in keeping matters in abeyance. In May, 1885, I got the following letter from him:—"Many thanks for your note referring to Mrs. X. It is so far satisfactory to hear that the tumour is not increasing in size or activity, and that she will probably be able to pass through the period of her pregnancy without the need of surgical interference. But I should fear that after her confinement the growth will become more energetic. Anyway, I am quite in accord with you that for the present nothing should be done. I should be much obliged to you if you would kindly let me know the ultimate issue of the case."

During the months of June and July the tumour somewhat increased in size; the circumference of the leg, measured at the most prominent part of the swelling, was found to be somewhat less than an inch larger than in February—nearly 12 inches. Except for this there was no other appreciable change—pulsations continued as strong over every part of the tumour, the bruit as loud as before, and there was no pain.

On the 20th of August, 1885, Mrs. X. was confined. Mr. Erichsen agreed with me in recommending that she should nurse her baby. This she did, but in November the menses, which had ceased, reappeared, and it became a question whether she should continue nursing. She communicated with me on this subject on December 15th, 1885. As during all this time there was no change save some increase in the circumference of the tumour, and as Mr. Erichsen and I could not help thinking that at first pregnancy and later lactation had at least assisted in staying the progress of the malady, we still recommended that Mrs X. should continue to nurse her boy, and she did so.

Some weeks after the birth of the child Mrs. X. had an attack of phlegmasia dolens in the good leg; the other limb, however, was not affected; the œdematous swelling was altogether confined to the left leg. On December 31st, 1885, the baby died. Owing to the attack of phlegmasia dolens, &c., the patient was very much confined to bed, and, in fact, for nearly six months was constantly more or less in the recumbent posture; during this time the elastic stocking was almost constantly worn.

On getting up after this prolonged delicacy Mrs. X. found that all pulsation had ceased, and that the tumour was smaller. As she did not reside in Dublin I had not seen her for some time. Great was my surprise when I next saw her in February, 1886, to find all pulsation gone, and the tumour unmistakably reduced in size.

I could hardly believe it possible that so formidable a disease—for I had regarded it as a pulsating periosteal sarcoma of the fibula—had undergone complete cure. I begged of the patient to let me see her again at intervals, and did not venture to write again to Mr. Erichsen until repeated examinations made me feel more confident that the trouble was not merely dormant. I did not therefore communicate with him until October, 1886, when I received from him the following reply:—"Mrs. X.'s case is most interesting, and I am greatly obliged to you for your kindness in sending me the details respecting it. The view that I took of the pulsating tumour was that it was a hæmatoid sarcoma, and that it would necessitate amputation. I do not remember ever to have seen a similar case. I have several times seen pulsating sarcomata of bone or the old osteo-sarcomata cease to pulsate, but then the tumour has developed rapidly, and rendered operation necessary. The remarkable, and to me novel feature in Mrs X.'s case is that, as you express it, 'the tumour is gone.' It will be very interesting to watch the case, and to see if this disappearance of the tumour is permanent; I should fear that it will not prove to be so. One remarkable feature of Mrs. X.'s case was the remarkably loud and shrill character of the bruit. This would of course cease with the pulsations."

Knowing that her case was an unusual one, and seeing the interest I took in it, Mrs. X. continued to let me see her at intervals. Gradually all traces of the tumour disappeared. She visited London in the spring of 1887, and I begged of her not to fail to see Mr. Erichsen. On May 9th, 1887, he wrote to me as follows:—"Mrs. X. called on me this morning; I examined her leg very thoroughly, and could find no trace of tumour, pulsation, or bruit. A most interesting case, and one that I think deserves record. Is it possible that the vessels which were in so active a

state when I last saw her had become plugged during the attack of phlegmasia that followed her confinement, or is the cure due to prolonged rest and slight compression?"

At my request Mrs. X. and her husband kindly called at my house on Tuesday last (November 6th, 1888). She is in excellent health, and there is no vestige of tumour, pulsation, or bruit. The circumference of the leg at the part where the pulsating swelling had been most prominent is now $10\frac{1}{2}$ inches ($12\frac{1}{2}$ in August, 1885, shortly before her confinement). It was not found possible to make any useful comparative measurement of the right leg, as this leg still continues considerably enlarged ever since the attack of phlegmasia.

Mrs. X. still wears the elastic stocking. As it is now approaching three years since all pulsation ceased and the tumour began to decline, I think we are justified in hoping that Mrs. X.'s recovery is complete, and will prove permanent.

Pulsating tumours connected with bone are not common, and in such cases, as everyone knows, the prognosis is unfavourable. Such cases as I now record must be rare indeed—where recovery follows prolonged rest, local pressure, and possible plugging of vessels—although it should be remembered that the limb on which the pulsating tumour was situated was not in any way visibly affected by the phlegmasia.

The cases of pulsating tumours of bone which I have myself seen are not numerous. One occurred in a patient of Dr. Hutton, in the Richmond Hospital, when I was his resident pupil. This was on the head of the tibia, and amputation was performed above the knee. Professor Robert W. Smith, at that time Curator of the Pathological Museum of the Richmond Hospital, examined the limb, and pronounced the tumour to be malignant. The patient made a tedious, but ultimately a good recovery. He may be alive still, but I am able to state positively that twenty years after he still survived, and had no return of malignant disease.

The interesting case recorded by Dr. Cruise in the Proceedings of the Pathological Society of Dublin, April, 1866, I also had an opportunity of examining. In this instance the lower end of the

femur was implicated, and amputation was resorted to. More than twenty years have now elapsed since this patient lost his limb, and I am glad to say he has had no return as yet. [Drawing and specimen exhibited.]

A case very similar to that published by Mr. Stanley, in his well-known paper in Vol. XXVIII. of the Medico-Chirurgical Transactions, occurred under my observation in the hospital of the Mountjoy Convict Prison, in former years, when I was Medical Superintendent of that institution. The patient was a well-made young man, twenty-five years of age. The tumour presented itself in the right sciatic notch. It caused intense pain along the sciatic nerve and its branches, but was at first so deeply seated beneath the gluteal muscles that no pulsation was distinguished; it grew rapidly, and ultimately filled the right side of the pelvis. It was found to pulsate, but gently and with a purring thrill, not with the "deep, heavy beat" described in Mr. Stanley's case. The *post-mortem* examination in this case proved it to be an osteo-sarcoma of intense malignancy. Bony spicula were found through every part of the enormous mass. All pulsation had ceased for a considerable time before death, which may be accounted for by the fact that the vena cava was found plugged by a large mass, which proved not to be a clot, but a growth containing spicula of bone and identical in microscopical structure with the tumour itself. [Drawing of this growth filling the vena cava presented for inspection.]

In connection with the foregoing, a very interesting and remarkable case, styled "Osteo-aneurysm," and published by Professor Mapother, should be referred to. This case was treated by the application first of potassa fusa, and afterwards by the actual cautery; it was brought under the notice of the parent of this Section of the Academy, "The Surgical Society of Ireland," in January, 1863. I have Professor Mapother's assurance that this patient has had no return of the disease, which was situated near the middle of the left tibia.

In conclusion, to return for a moment to Mrs. X.'s case—it was one of so much moment, as well as of so much interest, that I

examined the patient often, and very carefully. If I were now asked—Should you again meet with a similar case, what are the chief points on which you would rely to distinguish it from a pulsating malignant sarcoma? I should say briefly the slowness of growth, the situation—not at the epiphysial end of the bone, but on the shaft—but chiefly the strength of the pulsation and the loudness of the bruit. These last symptoms were unlike the gentle throb and the soft purring bruit which I have observed in the other cases of pulsating tumours of bone with which I have met. The last is a diagnostic symptom which did not escape Mr. Erichsen's acute observation; he observes, in his letter of October, 1886—nearly two years after he had seen the patient, and then only once:—"One remarkable feature of Mrs. X.'s case was the remarkably loud and shrill character of the bruit." This had taken a hold on his memory as something unusual.

NOTES ON A PECULIAR DISLOCATION OF THE THUMB.

By JOSEPH DALLAS PRATT, M.D., F.R.C.S.

[Read in the Section of Surgery, December 7, 1888.]

I WISH to bring under the notice of the Surgical Section of the Academy the notes of a case of lateral luxation of the ungual phalanx of the thumb, which I believe to be an injury of very rare, I might almost say unique, occurrence.

Dislocation of any joint is a much less frequent accident than fracture, the proportion from notes which I have myself taken in 530 cases being 510 fractures to 20 dislocations, or 25·5 to 1.

The particular dislocation to which I draw your attention seems to be so uncommon that we look in many text-books without gaining any information concerning it.

Erichsen says, "I do not think that simple dislocation of the ungual phalanx from the second is possible" (7th edit., vol. i., page 494). Bryant, that "dislocation of the ungual phalanx of the thumb may take place in either direction, forwards or backwards, the last being the more common."

Ashurst only speaks of the forward and backward luxations. Hamilton quotes one case, backwards. R. W. Smith makes no mention of luxation of this joint in any direction. Sir A. Cooper gives three cases, all either forwards, backwards, or compound.

A case of compound incomplete lateral luxation is given in the *Medical Times* of March 15th, 1862. None of these cases is, however, exactly similar to the present one.

Stimson, in his work on Dislocations, states that there are only four or five cases of lateral luxation of the ungual phalanx of the thumb on record, and these include the compound ones.

Polaillon, who gives the largest statistics on the subject with which I am acquainted—namely, 206 cases of dislocations of the phalanges of all the fingers—only states that 69 were of the terminal phalanges, and does not say how many were of the thumb, or what number were compound, neither does he state the direction of any of them.

From all these facts taken together, I think I have shown that the injury is extremely rare.

The subject of it, a man about forty years of age, came to me, his thumb presenting the appearance represented by the cast which I show.

He stated that he had slipped in the street, and putting his hand out to save himself, came down on the extremity of his thumb.

On manipulation, the base of the ungual phalanx was found to be in a position parallel to and in the same plane with the distal extremity of the proximal phalanx. There was no fracture, nor did any of the ligaments appear to be torn.

The phalanges, as you may see from the cast, were extremely short, and the question at once arose how reduction was to be effected.

Having secured a cast of the deformity, I tried several methods, such as a tape put on first with a clove hitch and then with several other knots, after that a Levis apparatus, all of which failed. I next tried grasping the phalanx with my forefinger and thumb, and bending the joint so as to bring it at a right angle with the next phalanx in a lateral direction and then making direct traction in the axis of the thumb. After one or two attempts this succeeded, and the joint went into its natural position with a characteristic snap.

The dislocation did not recur, and I saw the patient about a year after, when he told me the joint seemed as strong as before the accident.

ON EXOSTOSES OF THE EXTERNAL AUDITORY MEATUS.

By ARTHUR H. BENSON, M.A., F.R.C.S.;

Ophthalmic and Aural Surgeon to City of Dublin Hospital; Examiner in Ophthalmic and Aural Surgery, Royal College of Surgeons, Ireland.

[Read in the Section of Surgery, December 7, 1888.]

In February, 1885, I read the notes of a case of ivory exostosis in the external auditory meatus of a gentleman aged thirty-three. The growth was sessile, the skin over it was not ulcerated, nor was there any discharge from the ear.

The operation was done with a dental engine and completed with chisel and hammer. Result most satisfactory, and no return has since occurred.[a]

I now wish to record two other cases, each very different from this one and from each other:—

Case II.—P. D., aged forty-eight, a steward in County Wicklow, was sent to consult me by my friend, Dr. Symes.

The history given was—that for three years he had been deaf in the right ear. He had had otorrhœa for about twelve months, but had only recently consulted Dr. Symes in consequence of severe pain in the ear, and an excessively offensive odour from the discharge. Dr. Symes, seeing the case was one for operation, advised him to come up and consult me, which he did on the 19th October, 1887.

I found the right auditory meatus blocked up by a smooth, hard mass, covered with thin ulcerated red skin, which bled easily on being touched with a probe.

A very fine probe could be passed between it and the walls of the meatus everywhere, except anteriorly, where it was attached to the bony wall of the meatus by a bony pedicle.

Between the growth and the walls of the meatus a thin, most

[a] Transactions Acad. Med. Ireland. Vol. III. Dublin Journal Medical Science, April, 1885.

fœtid fluid forced its way. The growth was hard and perfectly immovable, and several attempts to pass the wire of a Wild's snare around it (for diagnostic purposes) failed.

On passing the probe along between it and the posterior wall of the meatus a very considerable quantity of thick, cheesy pus was removed in scraps. With the aid of the fine nozzle of a hypodermic syringe the collection was sufficiently cleared away to admit of inflation, by Politzer's method, and the air passed freely through. The membrana tympani was thus shown to be perforated.

In the right (affected) ear the hearing power, which had before been quite lost, was then found to be wh. at 15 cm., c.v. at 2 m. The left ear was unaffected, and had normal hearing.

He was admitted into the private ward in St. Mark's Hospital, and on the 29th October, 1887, with the assistance of my colleagues, Mr. Story and Mr. Arthur Baker, I proceeded to operate, the patient being under chloroform.

Having previously determined the exact position of the pedicle, I divided, with a blunt pointed knife, the skin and soft structures round the pedicle above, below, and externally. I then passed in a narrow chisel (for the use of which, and the hammer, I am indebted to Mr. Baker) along the wall of the meatus to the pedicle. A very few blows of the hammer sufficed to cut through the bone. The tumour was then grasped with a crocodile forceps, and easily removed, tearing through the skin on the internal surface.

The hæmorrhage, while cutting through the pedicle, was considerable, but never severe, and the whole operation occupied only about ten or fifteen minutes. The hæmorrhage was combated by frequent syringing with boric acid lotion. The exostosis, after being in spirit for a week, weighed 10 grains, and measured 14 mm. in length by 10 mm. in breadth. It was a tolerably regular oval in shape, with a few small bumps on its surface, which was otherwise nearly smooth.

The meatus was packed with boric acid powder, and left undisturbed for forty-eight hours. There was no pain nor were there other bad symptoms at any time after the operation, and on the third day he was allowed out, and returned home on November 4th—that is, within a week from the operation. The treatment of the otorrhœa and perforation of the membrana tympani was continued in the country; and I have seen him from time to time since. The otorrhœa had almost wholly disappeared when I saw him some months ago; and I understand that he is now practically well. No

return whatever of the growth appeared, and the discharge was perfectly sweet after a few days. The necessity for immediate operation here was imperative, as the discharge from the middle ear could not find a free vent through the external meatus, and had already accumulated and decomposed to a considerable extent.

Case III.—Mr. J. K., aged thirty-eight, from Clonmel, consulted me on September 24th, 1888, in consequence of deafness of the left ear, with considerable discharge, which had lasted for some nine months. I found the meatus occupied by a tumour covered with florid granulations; this I at first took to be a polypus, but soon discovered it to be an exostosis, the skin covering of which had ulcerated.

The meatus was completely filled by the growth, and even a very fine lachrymal probe could not be passed between the growth and the opposite wall of the meatus without bruising the parts and causing much pain.

After removal of the granulating surface with Toynbee's ring forceps, the shape and position of the bony growth could be better made out, though at all times there was great difficulty in accurately estimating the extent of the growth.

The meatus was everywhere narrow; at a point about one inch from the external orifice of the meatus a rapid narrowing took place, so that only a fine wire probe could be passed between the opposing walls of the meatus. This narrowing occurred both from the anterior and posterior walls, but principally from the posterior. Owing to the depth of the lesion in the meatus, the narrowness of the orifice, and the swelling of the soft parts, very little information could be obtained by looking into the ear, but with the aid of a probe the outline of the bony tumour could be traced with some precision. It had nothing corresponding to a pedicle at all, and was really more like a local narrowing of the bony orifice of the meatus. On its surface a small projecting mass could be felt, about 2 mm. in diameter; everywhere else the surface was smooth.

A somewhat similar projection occurred in my second case.

Above and below there was a small triangular opening, through which a fine wire probe could be passed to some depth (but not without pressing against the soft parts).

The condition of the membrana tympani could not be seen, but from the results of the use of the Eustachian catheter and Politzer's inflation, I inferred that it was not perforated.

The ear was very deaf; he could only hear loud conversation voice close to the ear. The other ear was normal.

Measuring the length of the meatus in the sound ear, and comparing it with the diseased one, I concluded that the narrowing took place half an inch in front of the membrana tympani. How far inwards it extended I could not tell, but from the general character of the formation I expect it must have extended almost to the membrane.

On October 16th, with the aid of my colleagues, Mr. Story and Mr. Arthur Baker, I attempted, the patient being under chloroform, to remove a portion of the posterior wall. The instruments I used were kindly lent to me by Mr. Baker; they were dental tools, and worked admirably in my second case. In this case, however, the conditions were different, and we had the very greatest difficulty in getting the narrow chisel to cut in the direction required, for, apparently, the density of different parts varied, and the chisel showed a tendency to turn on its axis. After working very cautiously for about an hour the growth was not loosened at all, though a number of small chips of bone came away both at the time and subsequently.

However, so much had been gained that a ledge had been cut in the smooth sloping surface of the growth, and a definite resting-place for any cutting tool was obtained. By this time the patient began to exhibit some unpleasant symptoms from the continued chloroform, so I desisted from further chiselling, packed the ear with iodoform powder, and next day sent him home to the country for a week.

In the interval, with the aid of Mr. Baker and Mr. Story, we devised a gouge, which I got fashioned as best I could in the time.

On October 25th we again attacked him (under chloroform); this time my friend Mr. Swanzy also came to add his experience to ours. The ledge, cut at the previous operation, afforded a fairly firm support to the gouge, and the curve of the edge of the instrument pretty well assured its following the direction of the wall of the meatus. Some firm hammering sank the gouge into the bony growth, and by shifting its position up and down, a deep slot was cut, and the piece of bone loosened. This was seized with a strong forceps and torn away. The soft coverings of it, left hanging in the meatus, were removed, and a probe could then be pretty easily passed in to the position where the membrane ought to be.

The orifice so obtained, though not large, was deemed large enough for purposes of hearing and to give exit to discharges. The

meatus was dressed as before, and in three days (there being no secondary inflammation of any account) he was allowed to go home to the country with directions as to treatment.

Hearing power slightly improved—c.v. at 0·5 m.

The portion of bone removed looks like a portion of the meatus, and is very different from the second specimen. It measures 9 mm. × 5 mm., and may be taken to represent about half of the total amount of bone removed.

There was necessity here for operation, as the meatus was wholly blocked up, and no external vent existed for the escape of any discharge which formed behind the growth.

I again saw the patient yesterday (and the members of the Section have had an opportunity of examining him themselves this evening). His present condition is :—Discharge entirely ceased; all soreness and tenderness in the meatus gone; health and spirits so much improved that he says himself he is a new man, and the hearing-power quite restored to the ear that was operated on, so that it is now as good as the other, in which he can hear a whisper across the room.

Dr. Charles H. Burnett, of Philadelphia, has reported before the American Otological Society [a] a case very similar to my second one, in which he removed the growth in pieces by means of a specially constructed bone forceps.

I do not think the method has any merits, and even the author speaks slightingly of it himself. It took him one hour and twenty minutes to perform.

To destroy tumours the size of those just recorded by electrolysis, or acids applied for the purpose of dissolving them, would be a tedious and painful task, especially where the bone was so dense as in these cases.

Much controversy has arisen as to the best method of operating on bony growths in the ear. Disputants should remember that the very greatest variety exists in the size, position, shape, character, and complications of such growths; and those who advocate only one method of operation are most probably those who have had but limited experience of the others.

[a] Trans. Am. Otol. Soc., July, 1887.

These three cases seem fairly representative of the types of exostosis or hyperostosis met with in the ear. And the method of removal in each case seems to have been well enough adapted for its purpose.

In the first case the growth was sessile, intensely hard, not ulcerated, and pretty near to the external opening of the meatus. There the dental drill, or saw, could be used to cut a deep slot in the ivory-like growth, and the chisel in this slot could be used without fear, and with excellent success.

I can think of nothing that would have answered better.

In the second case the growth was pedunculated, and the chisel cut through (or cracked off) the neck of bone quite satisfactorily. This growth was too deep in the meatus for working the dental drill with satisfaction unless specially long instruments were made.

In the third case the gouge was evidently the right tool. The dental engine could hardly have been used; and the chisel, though it did good work in preparing the way for the gouge, might not have been required if I had had a more suitable gouge at the beginning.

ON CHOLECYSTOTOMY.

By KENDAL FRANKS, M.D., Univ. Dubl.;
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[Read in the Section of Surgery, December 7, 1888.]

Although medical literature during the past nine years has recorded many cases of cholecystotomy, I am not aware of any case having hitherto been reported in Ireland in which the gall-bladder has been opened for the removal of gall-stones, or for the relief of obstruction of the ducts. I will therefore ask you to bear with me if I enter somewhat at length into the details of a case which came under my care in the Adelaide Hospital in the autumn of 1887, and in which I performed this operation.

Mrs. C., aged fifty-two, was admitted into hospital on the 18th of August, 1887, under the care of my colleague, Dr. Wallace Beatty. She was then suffering from attacks of fever resembling ague, and from violent attacks of pain, which were attended with jaundice and sometimes vomiting. Her previous history was as follows:—She had lived for years in India, where she was born. She came home in 1865. She had fourteen pregnancies and four miscarriages. Two years before her admission the catamenia had disappeared. During June and July, 1886, she went through a tedious and anxious nursing of seven children in scarlatina. In August of the same year, a son came home from India, and went out of his mind, and in September, another son, aged seventeen, died. To this succession of troubles she attributed much of her subsequent illness. From September to the end of December she felt weak and was troubled with a fulness in the epigastrium after her food, but she had no pain. On the 26th of December, 1886, she was attacked, for the first time, with pain across the upper part of the abdomen, of a severe character, and lasting for several hours. This attack was followed by jaundice. Since then she

had had several attacks, all of them attended with jaundice, and occasionally, but not invariably, with vomiting. The course of these attacks was always the same—varying only slightly in some details. First, there was the severe abdominal pain as described, lasting for some hours, which might or might not be accompanied with vomiting. Then jaundice supervened. During the attack aguish symptoms appeared, beginning with shivering, followed by fever, the temperature rising several degrees and terminating in a sweating stage. Great itchiness of the skin was complained of. Between the attacks, the jaundice either diminished or disappeared entirely. Constipation did not precede the attacks.

On her admission into hospital her condition, as noted by Dr. Beatty, was as follows:—She was very thin and deeply jaundiced. The pulse was 57, small and compressible. On examining the abdomen, an abnormal swelling or tumour was found on the right side, descending from the hepatic region and reaching nearly to the crest of the ilium. It was quite solid to the touch, and descended on inspiration. It was readily felt between the hands when one was placed posteriorly in the loins and the other anteriorly over the right side of the abdomen. It had a well-marked smooth free border on the left side slightly to the right of the linea alba. This tumour seemed to come from below the right costal arch, as the hands could not be sunk above it, between it and the ribs. No upper limit could be felt. She was provisionally ordered one drachm of sulphate of sodium, with 15 grains of bicarbonate of sodium, to be taken in half a tumbler of hot water half an hour before meals, and to get an enema of hot water with a drachm of ox-gall in it.

At 12 30 p.m. that day a very severe attack of pain came on across the upper part of the abdomen, with shivering followed by feverishness. During the pain the abdomen was again examined, and then a second tumour was discovered, quite distinct from the original one, a little to the right and below the umbilicus. It was small and round, about the size of a small orange, hard and painful, but not tender. As the pain subsided, this tumour became less distinct. That evening the enema brought away a copious motion, with scybala. No gall-stones were detected in it. On the following day, Aug. 19th, at 11 a.m., another attack of pain came on. At noon shivering began. At 2 p.m. the temperature was 101·8°, and the pain was less. Vomiting came on shortly afterwards, and continued almost incessantly until 7 p.m., when it ceased, and she

was free of pain. The vomited matter was green and watery. The temperature was then normal and the pulse 86. The following day, August 20th, a similar attack came on about the same time. The urine was examined. It was reddish, with a specific gravity of 1027; neutral in reaction, containing bile, but no albumen or indican. The small round tumour was again observed, but this time on a level with the umbilicus. For several succeeding days, a similar course of events was observed. On the 2nd of September, she had a fairly good day, except that she suffered from diarrhœa—six motions in the 24 hours. Towards evening an attack came on. At 6 p.m. the temperature was 104°; at 8 p.m. 102·6°; and at 1 a.m. 100·6°. Jaundice was pronounced. The diarrhœa continued for several days, during which time she had these attacks frequently. Morphin alone gave her temporary relief, and it became evident that, if unrelieved, she must, ere long, sink from exhaustion. We accordingly determined to make an exploratory abdominal section, and to see if anything could be done.

The diagnosis in this case was one of unusual difficulty, and in the absence of many members of the staff on their holidays, Dr. Beatty and I had the advantage of Dr. Walter Smith's advice and counsel. There were two tumours. Of the one occupying the right side, with a vertical left border, from the costal arch to very nearly the crest of the ilium, and which could always be felt, I was very uncertain as to the nature, and could not hazard an opinion; but it was believed by some who saw her that it was a malignant growth. As to the second tumour, I at first thought that it was a kidney, from its smooth and solid feel, but the day before operation I thought, most probably, that it was a distended gall-bladder—probably containing calculi.

On the 10th of September, 1887, the operation was performed. The patient was first put under the influence of chloroform, and narcosis was maintained with ether. An incision five inches long was made along the right linea semilunaris, and the peritoneum, when reached, was divided to the same extent. Having passed in my hand, I first explored the large tumour, and then found, to the surprise of us all, that it was an abnormal formation of a healthy liver. It was nothing more or less than a vertical lobe of the liver, evidently an extension downwards of the right lobe. It was quite healthy and did not require surgical interference. I may add that I was the more surprised because I had not hitherto seen such an abnormality, nor was I aware that such an one existed.

Being satisfied as to the larger tumour, I next explored the kidney. It was quite healthy, but it also presented a remarkable conformation, for the upper third of it was bent forwards upon itself, so as to present a concavity not only along its inner border, but also on its anterior aspect.

I now turned my attention to the gall-bladder, which was found without difficulty. It was somewhat distended, and was about the size of a goose's egg. On examining it with the fingers, several gall-stones could be distinctly felt within it. I then passed my fingers beneath it, and tracing it up to the duct, carefully explored the latter in order to ascertain if there was a gall-stone impacted anywhere in either the cystic or common bile duct. No such obstruction could be detected. The gall-bladder was now drawn into the abdominal wound, but before proceeding to open it I closely sutured the edges of the parietal peritoneum to the peritoneal wall of the gall-bladder, all round, apposing serous surface to serous surface. The remaining portion above and below of the original incision in the peritoneum was also closed with suture.

By these means a portion of the anterior wall of the gall-bladder, about the size of a half-crown, was exposed in the wound, and at the same time was completely shut off from the abdominal cavity. A small incision was next made into the gall-bladder, and at once a quantity of thin bile gushed out, and was caught in sponges. The incision was then enlarged so as to admit of a pair of forceps and a finger being introduced. Three large-sized and facetted stones were thus removed, and subsequently a small crescentic one. No more stones could be discovered, so the interior of the gall-bladder was irrigated with a warm solution of corrosive sublimate (1 in 2,000). A piece of thick mucus was seen floating by its free end in the fluid; this was caught in a forceps, and as it was gradually extracted, it came out as a thick mucous plug from the neck of the gall-bladder. It evidently had formed a plug helping to block up the cystic and common bile ducts. As no other obstructive cause could be detected, I proceeded to close the abdominal wound. This was done, by first suturing the edges of the opening in the gall-bladder to the skin, and then approximating the edges of the remainder of the skin incision. A large drainage-tube was inserted into the gall-bladder, and the wound dressed with the ordinary antiseptic dressings. The patient quickly recovered from the ether, and in the evening got a small hypodermic injection of morphin, after which she passed a tranquil night. She complained

of no abdominal pain, excepting a severe pain in the wound whenever she coughed, and which lasted for several days. This was evidently due to the fixing of the previously movable gall-bladder to the abdominal wall. The following morning the dressings were found saturated with bile, and had to be removed. That evening the temperature rose to 100·8° F., but next morning it had fallen to 99°, and did not again exceed normal limits during her whole convalescence. In fact it was always subnormal, from the third day onwards, with one exception, when it reached 99°.

On the third day the drainage-tube was taken out, cleaned and put back again; on the fourth day it was removed altogether. The entire wound healed by first intention, without the smallest attempt at suppuration, with the exception, of course, of the fistulous opening into the gall-bladder; and this is the more interesting, because during the whole period of healing the parts were constantly bathed in bile. We thus see that bile, in itself, in no way hinders the process of healing in a wound which is kept aseptic.

As there was no motion from the bowels up to the third day, the patient was given two grains of calomel in the morning, to be followed in the evening by an enema containing two ounces of castor oil and an ounce of turpentine suspended in two pints of water. This produced a copious evacuation, and thenceforward the bowels acted daily regularly. Some bile made its way into the intestine, as the fæces were slightly tinged; but by far the greater portion of it poured out at the wound. By degrees the opening in the gall-bladder gradually diminished, until it would not admit anything larger than a full-sized catheter. I made several attempts, by pressure, and variously-constructed pads, to prevent the bile flowing through this abnormal opening and to try and force it through the natural opening into the duodenum, but every attempt failed. The bile always forced its way past any obstacle which was applied, and it soon became clear that the patient was suffering from some organic stricture of the ductus communis choledochus. At the time of operation I satisfied myself by digital exploration that there was no impacted gall-stone, and no tumour present; so I have been obliged to conclude that there is a fibrous stricture present, which only allows a small quantity of bile to leak through it.

The patient left hospital on Nov. 12, 1887, with a fistulous opening in the side, which discharged almost the entire bile secretion. She was in very good health otherwise, and had put on a

little flesh. I again saw her last summer; she had greatly improved in general health, and had grown quite fat. The fistula still discharged freely, and although the bowels acted regularly, the fæces were scarcely at all stained with bile. From the day of operation until I last heard of her, she had had no return of the abdominal pain and no more aguish attacks.

The first operation for the removal of gall-stones, by opening the gall-bladder, seems to have been performed by an American, Surgeon Bobbs,[a] on the 15th of June, 1867. The feasibility of the operation had previously been suggested by Thudichum[b] in 1859. In Dr. Bobbs' case, a previous diagnosis as to the nature of the case had not been made, and it was not until the gall-bladder was opened that the true state of affairs was realised. The patient recovered. The first pre-determined cholecystotomy was performed by Marion Sims on the 18th of April, 1878,[c] but unfortunately the patient collapsed. The third operation of this kind was by Kocher in 1878,[d] and was perfectly successful; the fourth was by Lawson Tait,[e] and was performed on the 23rd of August, 1879, and to him, more than to his predecessors, is due the credit not only of having made this operation a recognised surgical procedure, but of having placed it "within the first rank of modern operative proceedings."

Since 1879, the operation has been performed in every part of the world, and experience has shown that it is by no means a very fatal proceeding, although no surgeon hitherto has been able to reach the brilliant standard attained by Mr. Lawson Tait. In the *Lancet* of April 14th, 1888, he shows that he has operated forty-one times with only two deaths, and neither of these could well be attributed to the operation.

Arguing from these statistics, he says:—"It is perfectly clear that when there in an absence of cancerous complications, and the age of the patient is such as to give her a fair chance, the recovery

[a] Trans. Ind. State Med. Society. 1868. P. 63.
[b] Brit. Med. Journ. 1859.
[c] Brit. Med. Journ. 1878.
[d] Corr. Bl. für schweizer Aertze. Bâle. 1879.
[e] Lancet. 1879. Vol. II., p. 730.

from this operation is almost certain, and the amount of relief obtained from it most amply compensated for the small amount of risk."

In the case which I have just detailed, I think all will agree that the operation was not only justifiable, but urgently needed. To those who have followed the history of the case up to the morning of operation, I think it will be clear, as it was to all those who had an opportunity of seeing the patient at that time, that her life was in the greatest danger; and even those who thought that the larger tumour felt was of a malignant character, did not hesitate to sanction an exploratory operation, so critical was her condition. But even when life is not immediately threatened, the agonising attacks of biliary colic are proverbial, and surgeons at any rate will be grateful that they have at last a method of treatment at their command, which in itself is not more dangerous in the long run than the treatment by morphin and the warm bath, and is productive of infinitely greater relief.

Before bringing this paper to a close, I would like shortly to call attention to a physiological point of some interest. We have been accustomed to look upon the bile as of some importance in the digestive and other processes which take place in the intestinal canal. We do not attribute to it an importance of the first order, as was claimed for it in former days, but at least we believed that it had some uses—and this has been enlarged upon in many works on physiology; but this case, and some other somewhat similar cases which have been published, show that patients enjoy the best of health, digest their food admirably, if we are to judge by their improvement in weight and healthy colour, and are not specially afflicted with constipation, when very little, and sometimes no bile at all, makes its way into the duodenum. From such practical experiences, I think we are led, logically, to conclude that the bile is an excretion, and is of little, if of any, use as a secretion, in the economy of the human body.

THE TREATMENT OF TUBERCULAR DISEASE IN AND NEAR JOINTS.

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[Read in the Section of Surgery 18, January, 1889.]

The cases which I intend to bring under your notice in this paper illustrate the radical methods of dealing with tuberculosis in and near joints. I omit all cases of resection, confining my remarks to the treatment of—

1. Scrofulous tendo-synovitis.
2. Acute and chronic tubercular osteomyelitis.
3. Circumscribed articular disease.

The necessity for the early and complete removal of diseased tendon sheaths and peri-articular deposits of a decidedly tubercular nature, is admitted by all who have discussed this subject, and is carried out by all who have faith in modern surgical methods. But in reference to the treatment of such cases as those I have detailed, of joint tuberculosis, there are many points still under discussion. I bring them before the Academy for the purpose of eliciting the opinion of members of this Section whose experience of such cases must, of necessity, be greater than mine, so that in future I may benefit by that knowledge in selecting arthrotomy, arthrectomy, or resection, for application in cases which come under my notice.

Owing to the introduction of the Listerian system of wound treatment, a great stimulus has been given to the study of tubercular disease, especially when primary and in the neighbourhood of joints. The safety with which exploratory incisions can be made has led to the recognition of the exact site occupied by diseased centres, and we have been enabled to study (without the danger of

septic infection) the changes which take place in these centres, from the initial inflammatory lesion to the destruction of the tissues engaged.

The experience thus gained has convinced most surgeons that the palliative treatment of advanced tuberculosis of bone, tendon sheath, or joint (especially in adults) is highly unsatisfactory, and has brought into vogue the vigorous operative treatment now generally, and very successfully, adopted.

The importance of the knowledge gained by early exploratory incision can hardly be exaggerated, when we contrast the results of the present system of treatment with those obtained by the most carefully regulated palliative measures. The tediousness alone of the latter would condemn it; but, add to this the fact that the local trouble is a constant source of danger, since it may at any time cause a general infection, and who will doubt but that thorough eradication of the diseased centres is the only reliable treatment of the affection. "General tuberculosis," says Bull, "is the product of a self-infection arising from a caseous centre." If this cheesy mass be in a position accessible to the knife it is rational to expect that (while supporting the constitution, and thus raising a barrier against fresh outbreaks of disease) the only real remedy for the disease itself is its complete removal, since there is only faint hope of rendering it innocuous by any other treatment. The old methods of treatment invariably failed unless nature helped by causing an expulsion of the centres by suppuration, or prevented their deleterious effects by encapsuling them, or causing their calcification. Most of the cures of local tuberculosis by the expectant treatment have been only apparent. The local trouble may have been gradually removed by disintegration and absorption, or suppuration and expulsion, but Cohnheim has noted that local caseous centres have become the source of general tuberculosis long before they can be destroyed by such treatment as counter-irritation, strapping, injection, &c. When we consider the usual situation and ordinary course of tubercular growths, little doubt will remain of the expediency of their early and complete removal.

The following are abstracts of notes of cases operated on by me

for scrofulous disease, chiefly in connection with tendon sheaths, bones, or slightly engaging joints. I publish them for the purpose of showing the possibility of preventing the extension of mischief by proper surgical interference, and the value of radical surgical intervention even in advanced disease. For convenience of comparison I have divided the list according to the location of the disease, and irrespective of dates. I have placed cases not alone according to the situation in which the disease occurred, but according to the tissue which seemed to me primarily and chiefly affected.

Section A.—Tuberculosis of Tendon Sheaths.

I wish first to call your attention to some cases of tuberculosis of tendon sheaths. This is a subject to which few surgeons have directed their attention, but all those who have communicated their results agree, that to be effective, treatment should be thorough and early. When I speak of tendon sheath tuberculosis, I mean the condition arising from the presence of primary disease in the synovial membrane of those sheaths.

Berger, in an able paper on this subject (*Deutch. Zeit für Ch.*, 1886, p. 335), shows how rapidly destructive the disease is, and how frequently success attends early and complete removal of the affected tissues.

If we watch carefully the course of such cases, we find that at first slight loss of muscular power and deformity are the only effects of the disease, but later we find it travel through connective tissue, ligaments, &c., engaging the synovial membrane of neighbouring joints, and even at times infiltrating subjacent bones, so that ultimately a disease, originating in a tendon sheath, may necessitate a resection of a joint, or even an amputation, as in Case X., Sect. A.

I have frequently seen specimens laid before this Academy in which the bones removed by resection at the wrist and ankle were free of disease, and the following engraving shows how tubercular disease of the tendon sheaths may simulate carpal and tarsal osteitis. Fig. 1 is from a cast taken three days before operation in

Case V. of the first series, and it shows a condition which rendered the hand useless. A thorough scraping away of numerous cheesy masses from the dorsal tendon sheaths was followed by a complete cure of the local trouble.

The following are brief notes of the cases on which I operated up to January, 1886 :—

Case I.—*Synovitis of Sheaths of Flexors in front of Wrist.*—Thomas D., aged twenty-three years, a farmer, came under my care on the 10th of October, 1882, suffering from severe pain and considerable swelling on anterior aspect of right wrist. Some six months previous stiffness and pain commenced, both being in the beginning most marked whenever he attempted to lift heavy weights or move the wrist quickly. Soon the pain prevented his using the hand, and relief was sought by fomenting and poulticing the arm. This temporarily soothed him, but finding the swelling increasing in size, and becoming more firm, he came under my care, the condition being as follows :—He was in vigorous health, having been to the seaside for three months prior to his visit to me. Right forearm wasted, muscles thin and soft; wrist two and a half inches greater in circumference than left; a mass rounded at both ends projected upwards two inches into the forearm, and downwards into the middle of the palm, where it appeared nodular and semi-elastic, with a sense of deep fluctuation.

Operation.—An incision was made, extending from the upper edge of the swelling downwards to the centre of the palm, going through skin, superficial fascia, upper half of anterior annular ligament, and deep fascia. Into the wound a great quantity of fungous tissue immediately projected. On examining the structures carefully I found that the tendon of palmaris longus, and the index and ring tendons of the flexor sublimis ended abruptly at upper part of swelling. The remaining flexor tendons and the median nerve were close to the bones and interosseous membrane. I carefully dissected and scraped away the fungous tissue, passed a large drainage tube under the remains of the annular ligament, and closed the wound by three rows of suture—one fine catgut drawing together round the tendons the deep connective tissue; a second stronger suture (catgut) drawing the edges of deep fascia and annular ligament; the third (silk) drawing the edges of skin together.

1st dressing, 6th day.—Sero-sanguineous discharge; drain placed in upper angle of wound, and hand supported high on chest.

2nd dressing, 15th day after operation.—Wound healed except at upper end, where tube was placed. Silk sutures removed; only slight suppuration in track of tube. Passive motion commenced, the wound being dressed with lint and iodoform ointment.

The patient improved in strength from this date, and at the end of the third week the wound was completely and soundly healed. One month after operation active motion was commenced, and now, six years after operation, he has a perfectly useful hand, the only inconvenience he suffers being a slight heat and aching in the wrist after severe exercise.

Case II.—*Synovitis of Flexor and Extensor Sheaths from Ulnar Tuberculosis.*—Thomas C., aged twenty-eight years, consulted me about a nodular swelling along the flexor carpi ulnaris tendon. He gave me the following history:—Three years since he noticed a slight painless swelling along the inner side of forearm close to the wrist. After a time there was stiffness at wrist, and gradually swelling appeared on dorsum of hand and in front of wrist; the hand then became useless, and he went under treatment in one of the Dublin hospitals, where poultices were applied continuously for months, until suppuration occurred at the upper part of the swelling, and pus was discharged through a ragged opening, the mark of which remained. This relieved him for a time, but still he could not make use of the arm. At irregular intervals pus would be evacuated through a small sinus which remained where the first abscess opened. This state of things continued until he consulted me on Dec. 7th, 1883, when his condition was as follows:—

He was pale and anæmic; pupils dilated; pulse quick and irregular; respirations rapid, but no evidence of disease of lungs. He was unable to use his left hand, as he found that after exercising it an aching pain extended from the swelling downwards along little finger. This was specially noticeable in the evening. The flexor and extensor sheaths had become distended, as in serous ganglionic enlargement, and a dense semi-elastic swelling surrounded the lower end of ulna.

Operation.—Incision along inner edge of ulna down to bone; retraction of edges; scooping out of large masses of fungous tissue; evacuation of serous fluid from tendon sheaths by free incision; drainage-tube laid in dorsal and flexor sheaths; ulnar wound plugged with iodoform gauze.

First dressing, 7th day.—Swelling almost gone from hand; ulnar

wound granulating; drains removed; iodoform to ulnar wound again, and Lister's dressing to hand and arm.

Second dressing, 17th day after operation.—Flexor and dorsal wounds firmly healed; ulnar wound filled with healthy granulation tissue. The ulnar wound was now dressed with zinc and carbolic ointment, and the patient sent to the Convalescent Home at Linden. When I next saw him, which was in the 6th week after operation, a firm cicatrix marked the position of the ulnar wound. I now allowed him to use the hand carefully, and in three months he called on me to say that his hand was as useful as ever it had been.

In February of 1886 I was asked by one of my colleagues to see a patient who was under his care suffering from pleuritic effusion and tubercular pneumonitis, and found that this was the poor fellow from whose arm I had, two years before, removed the mass of tubercle above referred to. There was no history of phthisis in his family, and I concluded that the poulticing had increased the local development of the disease, while the delay in removing it had allowed a general dissemination, which ended fatally.

Case III.—*Tendo-Synovitis, Radial and Ulnar Sheaths.*—K. M., aged twenty-six years, a general servant, came under my care with semi-elastic, and in parts doughy, swellings along flexor carpi radialis, flexor and extensor carpi ulnaris. She was unable to use the arm, and had been so for some months. Three weeks before she came under my care, the radial swelling was opened, but only blood escaped. As in the other cases, I cut down on, and scraped away, cheesy masses which surrounded the tendons, and dressed the wounds with iodoform gauze. In five weeks from the date of operation she left hospital with a useful and painless arm.

Case IV.—*Tendo-Synovitis of Extensors of Thumb.*—T. G., aged eighteen years, came to me from Queen's County, giving the following history:—Some six months before he consulted me he received a knock on the lower third of the radius on its posterior aspect. For some time this caused him no inconvenience, but three weeks or so after the injury he found pain and stiffness at the wrist, and difficulty in using his thumb. Five weeks after the injury he was quite unable to use his hand, owing to stiffness and pain at wrist. In the course of the next few months, notwithstanding the application of soothing dressing, the pain became very severe, and he came

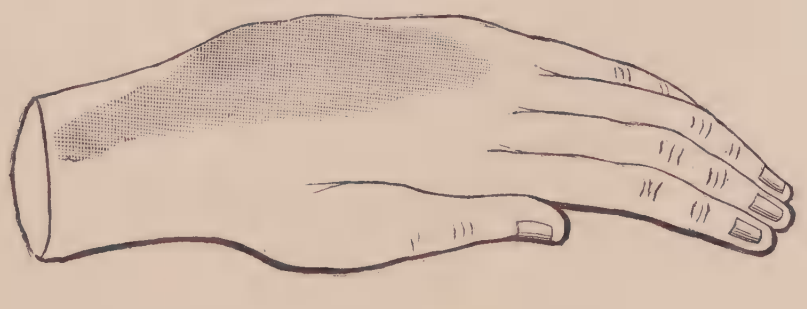
to me in the following condition:—One inch from the lower end of the left radius, a swelling extended for two inches along the posterior aspect of the bone, and extending inwards towards ulna. On the anterior aspect the tendons were pushed forwards by a semi-elastic swelling in the interosseous space towards the radial side. There was a general fulness at the wrist and inner side of hand, and the boy carried it resting in the other palm, as pain occurred whenever the hand hung down.

Operation.—I made an incision three inches in length on dorsal aspect of forearm over the most prominent part of the swelling, which I found to be, as I expected, a mass of tubercular granulation tissue, which passed downwards along the sheaths of the extensors. I scraped this material away thoroughly, and drawing the tendons to the ulnar side of the forearm, laid bare the interosseous membrane, which I found penetrated by the fungous tissue. I now made an incision on flexor aspect of radius, and drawing the tendons inwards, found a mass, similar to that already removed, engaging the anterior surface of the radius, and pushing forwards into the anterior tendon sheaths; this I scraped away, and passing a drainage-tube through the interosseous space, I plugged both wounds with iodoform gauze, and surrounded all with a Lister's dressing.

The wound healed rapidly by granulation, and the lad left hospital without pain or tenderness in the arm, and with all the motions of the wrist and fingers free.

Case V.—*Dorsal Tendo-Synovitis, Hand.*—Mary H., aged sixteen years, sent to me from Monaghan, with the following history:—Her father had died of phthisis; her mother was fairly healthy, but two of her sisters died of what appeared, from the history, to be mesenteric disease. The hand presented the appearance shown in Fig. 1, and was perfectly useless. Free dorsal incision, drainage, and the use of ethereal solutions of iodoform resulted in a rapid cure.

Fig. 1.



Case VI.—*Tuberculosis round Tendo-Achilles.*—P. R., aged nineteen, came to me on the 10th of December, 1880, complaining of pain along the back of his right leg, and swelling behind his ankle. On examination I found a doughy mass, with here and there spots of indistinct fluctuation; motions of the foot caused a creaking sensation, and at the insertion of the tendon a rather firm projection was found, which was painful on pressure, and whenever he walked any distance this spot became larger and more painful. The patient was strumous looking.

Operation.—Incision three inches, along outer side of tendon, scraping away of cheesy deposit, and of bursa, which was the cause of prominence at insertion of the tendon.

This case was slow in healing, and fungous granulation tissue filled the wounds. Compresses saturated in iodoform solution was applied, and in the course of six weeks the parts were soundly healed. There has been no recurrence, and the patient is at present strong and well.

Case VII.—*Tuberculosis of Sheaths of Peronei.*—Henry D., aged fifteen years, came under my care on the 14th of November, 1883. He was pale and anæmic, but not strumous looking. He enjoyed fairly good health until the summer of 1883, when, after twisting his right foot, he was for some time laid up, and got very thin. At that time he had a dull, aching pain on outer side of foot, and considerable swelling. After a time this subsided, and when I saw him a fusiform swelling extended from behind the outer malleolus, downwards and forwards, to the edge of foot. It was semi-elastic, with doughy spots, and here and there pain was occasioned by pressure or attempts at walking. Here free laying open and dissection of the sheaths of the peronei was followed by rapid cicatrisation, a useful foot resulted, and the boy's health improved immediately.

Case VIII.—*Tubercular Synovitis of Sheaths in front of Ankle.*—Mrs. H. consulted me early in March, 1884, about a fulness in front of the left ankle. She complained of great stiffness after much exertion, when the ankle and dorsum of foot became red and more swollen than usual. Counter-irritation, strapping with ammoniacum and mercury, proved useless, and when I saw her in June the swelling was greater, and at a point corresponding to the line of the tibialis anticus, an ovoid semi-fluctuating swelling existed, tender

on pressure, and apparently connected with a small round mass, which appeared under the head of the astragalus. Believing there was a collection of curdy pus here and fungous synovitis present, I made an incision from the middle of the front of the ankle, downwards and inwards, to the nodule under the astragalus. I scraped and dissected a large mass of fungous tissue from the inner aspect of the foot, and from round the tendons passed a drainage-tube across the foot under the tendons, and, closing the upper part of the wound, plugged the lower part with iodoform gauze.

The upper part united by first intention, and the lower part was firmly cicatrised in the second month after operation.

Fig. 2.

Case IX.—*Case of Tubercular Tendo-Synovitis at front of Ankle.*—Mrs. F. came under my care early in September, 1885. She had suffered a good deal from pain and stiffness in her left ankle, the stiffness being greatest in the morning, and in the evening pain being more troublesome. On examination I found that a rounded fluctuating mass existed in front of the ankle, extending to the edge of the outer malleolus, where it was most prominent. None of the tendons could be felt, and the case is well represented in Fig. 2,

taken from a cast of the foot. It seemed a case of tarsal osteitis, but hen I cut down on the bones, which I did immediately, I found that peri-osteitis had been excited by the presence of extensive caseous deposit all round the anterior tendons. Cheesy and fungous material were thoroughly removed, the upper part of the wound closed by deep catgut, and superficial silk suture, a large drainage-tube laid in, and carbolic dressing applied. This patient made a rapid and permanent recovery.

Case X.—*Fungous Tendo-Synovitis in front of Ankle; Operation delayed; Extension to Tarsal Synovial Membranes.*—Mr. O'H. came under my care on the 10th of April, 1885, with stiffness of his right ankle, following on a rather severe sprain received two years before.

Present Condition.—There is a diffuse swelling over the front of the ankle, slightly tender on pressure, but at no spot is fluctuation present. The tendons are embedded in a mass of semi-elastic tissue, but there is no evidence of bone disease. I advised free incisions, as he had already tried all the palliative measures, including firing, the marks of which were still present. The patient, however, preferred to again try counter-irritation, and I saw no more of him for four months, when he returned from the country with well-marked tarsal synovitis. I performed Syme's amputation, and found that a tubercular tendo-synovitis had led to a general inflammation of the synovial membranes of the foot and ankle. The bones were sound, but slightly inflamed. Here was a case in which early operation would have saved the foot, as it would have been possible to remove all the diseased structures at the time I first saw him.

Section B.—Tuberculosis of Bone.

When tuberculosis occurs as a primary disease at the articular ends of long bones, it is all-important to attack it immediately, as it often develops with amazing rapidity, producing destructive joint lesions, some of which are illustrated by the following woodcuts. Fig. 3 shows where an intra-osseous tubercular deposit in the lower end of femur caused destruction of a limited portion of the articular cartilage of the outer condyle, rendering excision of joint necessary. Now some months before starting of the limb occurred in this case, trephining of the outer condyle and drainage would,

in all probability, have saved the joint. Fig. 4 shows how a tubercular growth along edge of inner condyle of femur became subchondros, and caused detachment of the articular cartilage, rendering resection necessary. This mass was at first a small nodule on inner condyle, and, as seen in the woodcut at A, it invaded the bone as well as destroyed the cartilage. From the foregoing it will be seen that the process from the beginning is of a destructive nature, and, as pointed out by Ziegler, "The vessel formation does not keep pace with the tissue growth, there is no proper tissue formation, the granulations assume a fungous character, the pus corpuscles, epithelioid and giant cells, which under ordinary circumstances would have been used for the formation of connective tissue, combine now to form vessel-less, and therefore short-lived nodes." Such masses, compressing, infiltrating, and occluding vessels lead to destruction of the tissues in which they develop, or into which they may extend, with certainty if not rapidity.

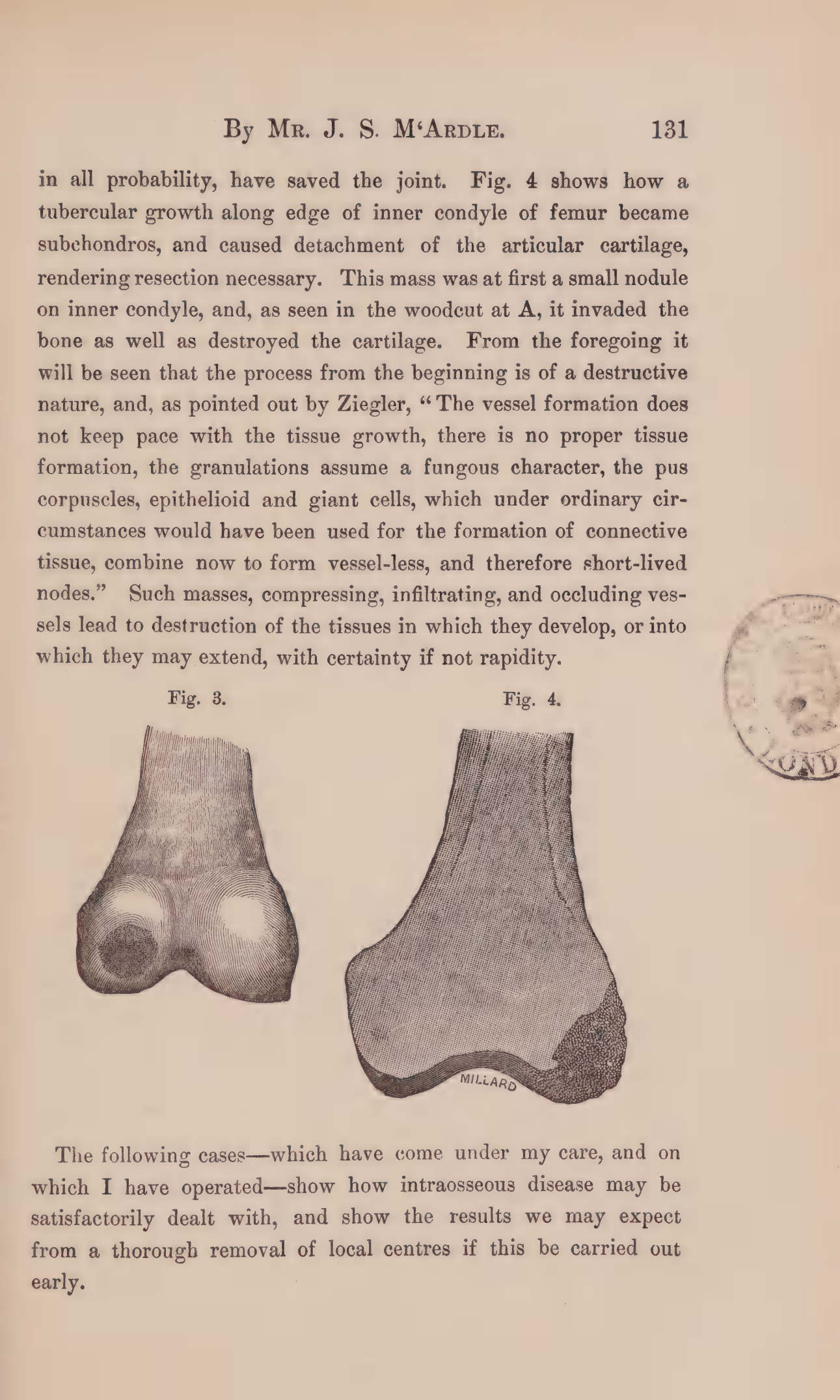


Fig. 3.

Fig. 4.

The following cases—which have come under my care, and on which I have operated—show how intraosseous disease may be satisfactorily dealt with, and show the results we may expect from a thorough removal of local centres if this be carried out early.

Case I.—*Chronic Osteo-myelitis at Lower End of Tibia; Trephining; Cure.*—H. F., aged twenty-nine years, a farmer, came under my care on the 11th of February, 1882. He had just left another hospital, where he had been under treatment for eleven weeks for pain and swelling in the left ankle. He gave the following history:—In the winter of 1880 he got his left leg and ankle severely crushed by a horse falling and catching it under him. Swelling and pain set in immediately, and for six months he was unable to raise his foot. By the end of 1881 he was able to get about fairly well, but after much exertion the leg felt heavy, and a dull aching pain in the ball of the great toe and in the front of the ankle caused him much uneasiness. After a long walk on a cold damp night in October the aching gave place to a violent lancinating pain shooting downwards to the toes and upwards to the back of the thigh; swelling again set in, and after trying every means he knew of getting rid of the pain, and without permanent result, he was sent by his attendant to hospital, where morphin was given to him sufficiently often to allay the pain, while poultices, stupes, counter-irritation, and strapping were tried locally. After eleven weeks of such treatment, he was recommended to have his ankle-joint removed, or to submit to amputation, as the entire ankle and foot seemed to be the seat of very extensive disease. On carefully examining the ankle I found the extensor tendons pushed forwards by a firm swelling, apparently of the tibia, which projected forwards, making the foot appear as if displaced backwards. The foot was œdematous, but there was no sense of fluctuation, nor was there any irregularity of the bones; by fixing the tibia the fibula could be found to be rather loosely attached at its lower end. The distance between the tips of the malleoli was greatly increased, as also the antero-posterior measurement of the tibia. This thickening of the tibia extended upward three inches. The entire surface of the lower two-thirds of the limb was red, glossy, pitted on pressure, and was extremely painful and tender. Unless when raised, there was throbbing in the foot and leg, which became unbearable if the patient stood up and allowed it to hang down. With this local condition existing so long, the patient had become worn to a shadow, was excessively irritable, with a small, quick, irregular pulse, rigours, frequent vomiting, and copious perspiration; temperature ranging from 99·6° in the morning to 103·4° in the evening. In such a condition I found him, and on the day after I cut down on the tibia on its inner aspect, raised the periosteum

so as to admit a 16 mm. trephine, which I passed deeply into the bone. On withdrawing it blood oozed rather freely from the thickened bone around. This, however, soon stopped. On examining the piece of bone removed, I found that I had reached cheesy material. I now reflected light into the bone, and saw that a cheesy mass pressed into the trephine opening. With a scoop I cleared out the entire collection of tubercular material, which evidently started in the tibia, but had engaged the interosseous tibio-fibular ligament, and passed into the lower end of fibula. I plugged the cavity extending into the outer malleolus with iodoform gauze, applied antiseptic dressings, and had the limb elevated. During the evening there was some pain, and the patient complained of the pressure of the dressings. This inconvenience, however, soon disappeared, and on the third day it was found that the dressings were almost saturated with a serosanguineous discharge. The temperature fell from 103·4° to 101° immediately after the operation, and at no time after the second evening since operation did it reach 100°. The dressings were very carefully conducted; and on examining the ankle four weeks after operation, the wound was soundly healed, and the patient could perform all the movements of the foot with perfect ease.

I have frequently seen the patient during the past few years, and I am happy to say that although he works hard, carrying heavy weights and walking long journeys, and standing in fairs and markets, not the least trace of pain, stiffness, or uneasiness results from his exertions.

Case II.—*Extensive Tubercular Deposit in Lower End of Tibia, causing Synovitis of Ankle; Trephining; Cure.*—John C., aged thirty-four years, came under my care on the 20th of September, 1883. Some ten months previous he slipped when crossing a wall, and fell heavily on the left foot. After a little time he was able to walk about without much uneasiness, until three months or so after the accident, when pain occurred in the instep and swelling appeared at the ankle. This was treated by liniments, strapping, and blistering; but the pain continued, the swelling increased, and when I saw him the ankle was in the following state:—there was a reniform fluid swelling round outer malleolus; the anterior tendons were pushed forward by fluid collected in the ankle-joint; the lower end of tibia was nearly twice the normal size; was tender on pressure, which caused pitting; and when allowed

to hang down, throbbing pain occurred; the motions of the ankle were limited to the slightest amount of flexion and extension; the leg was wasted, and the foot cold and clammy.

Operation.—Incision two inches in length on middle of inner malleolus; large trephine passed through tibia and into fibula; scraping away of extensive tubercular deposit in end of bone; plugging with iodoform gauze.

The wound healed by granulation in five weeks, but, one month later, pain again set in, and, after fomenting and poulticing for two days, the cicatrix opened at one end, and gave exit to about one drachm of healthy pus. Rapid healing now took place, and since that time the patient continues well.

Case III.—*Acute Tubercular Osteomyelitis at Lower End of Tibia; Perforation of Articular Cartilage; Suppurative Synovitis of Ankle-joint; Trephining; Irrigation of Joint; Cure.*—Thomas R., aged nineteen years, came under my care on the 7th of May, 1884, in the following condition:—His right ankle, which one month before had become suddenly painful and swollen, was one and a half inches greater in circumference than the left; it was tender, red, and glossy. Pitting on pressure occurred as high as the upper third of tibia. Any motion of the joint caused great pain; and pressure on the sole of foot could not be borne, painful startings of the limb occurring at each attempt at laying any weight on the foot. The local temperature was 3° higher than on the opposite side—shivering frequently occurred—and the general temperature was 99° at 9 a.m., 102·6° at 7 p.m.

Operation.—Free incision down to bone; elevation of periosteum; large trephine passed into bone; scraping away of pus and cheesy deposit, which showed that the cartilage was perforated about the centre of the tibial articular surface. Through this aperture pus welled up into the trephine opening, and, by moving the joint, a large amount was evacuated. The joint was washed out with warm carbolic solution, 1 per cent., the cavity in the bone plugged with iodoform gauze, and carbolic dressings were applied. For two months these dressings were continued, at intervals of from five to ten days, the swelling gradually subsiding, and the wound filling with healthy granulation tissue. One evening, when healing was almost complete, after some unusual exertion, throbbing set in at the site of trephine operation. A few poultices caused a discharge of pus, which ceased in a few days, and from this time healing was uninterrupted.

Case IV.—*Tubercular Osteomyelitis at Lower End of Tibia; Suppuration; Perforation of Ankle-joint; Trephining of Tibia in hope of saving Foot; General Tarsal Synovitis, which does not subside; Symes's Amputation; Healing.*—Laurence T., aged twenty-one years, consulted me in the summer of 1884 about a stiffness and swelling at the right ankle. Tibia was greatly enlarged, and, at its inner aspect, tender on pressure. I advised operation, but he wished to see how other treatment would affect the joint. In the December of the same year he came under my care for operation, in the same condition as Case III., swelling being more marked in the tarsal region. Operation as in last case. The entire lower end of tibia was softened, and the articular cartilage destroyed. After a few days' treatment, as in Case III., it was found that the disease had spread too far to be checked, and amputation became necessary. The case did well, but early operation would have saved the foot.

Case V.—*Tubercular Osteitis at Lower End of Tibia; Synovitis of Ankle, and Tendon Sheaths; Trephining; Cure.*—Miss M. O'B. came under my care with symptoms as in Case III. Operation and after-treatment similar. Operation, 7th of November, 1886. Complete healing, January,1887—since which time there has been no return of swelling or pain.

Case VI.—*Chronic Osteitis at Upper End of Femur; Trephining; Cure.*—Ml. H., aged twenty-six years, came under my care in August, 1883. For three years he had suffered from pain and stiffness in the right hip. On examination I found that the great trochanter and upper third of femur were much enlarged, pitting on pressure occurred at middle of outer side of thigh, and there was filling up of the ischio-trochanteric fossa. Pain was referred to the outer side of knee and to the sciatic nerve and its divisions. Any attempt at moving the thigh caused spasm of the adductors. The leg was flexed and adducted, and had been so for months. Under chloroform the shortening disappeared, and the movements of the joint were almost normal.

Operation.—Free incision from upper end of great trochanter, 3 inches in length, down to bone; elevation of periosteum below most prominent point of great trochanter, where a trephine opening in the bone gave exit to a drachm or so of curdy pus. With a large scoop the neck of femur and the trochanter were freed of a large amount of fungous granulation tissue, with many pieces of necrosed bone; plugging with iodoform gauze; drainage; carbolic

dressings. The temperature reached 101° on the evening of the second day, but after that declined to normal on the fifth, after which date healing went on without delay. In this case the limb was placed in the position of extension in forty-eight hours after operation, without causing pain. The patient is now able to be about, and does not suffer the slightest uneasiness. The limb has regained much of its strength.

Case VII.—*Chronic Osteitis at Upper End of Femur; Large Opening in Bone; Evidement; Cure.*—Mr. P. B., aged fifty-two years, came under my care on the 20th of February, 1886, in the following condition:—The limb was shortened $2\frac{1}{2}$ inches (and had been so since he was twenty years of age. At twelve he had morbus coxæ, but shortening became gradually more marked, until it had reach the extent specified). The great trochanter was much thickened, tender on pressure, and at its posterior aspect a sinus led towards the back of the neck of the femur. From this sinus a thin fœtid pus discharged. In all directions from the opening of this sinus cicatrices could be seen marking the site of operations which had been performed for the purpose of healing it. There was deep pain in the hip, and pain extended along the outer side and back of thigh, and after walking any distance a throbbing pain was felt in the hip. Believing that central osteitis was the cause of the trouble, and that the posterior part of the femoral neck had been perforated by the spreading disease, leading to the formation of abscess, the opening of which was followed by the formation of the sinus referred to, I laid bare the upper third of the femur, and with a chisel removed a piece from its outer aspect, extending one inch downwards from the most prominent part of the trochanter. Through this opening I evacuated with a large scoop a considerable quantity of necrosed tissue and some curdy pus. The sinus leading to the neck of the femur I scraped with a small sharp scoop. The bone cavity and the sinus were now plugged with iodoform gauze, and during the course of the case dressings were conducted as in the other cases of trephining, with strict antiseptic precautions, at intervals of five to nine days as was necessary. An uninterrupted recovery was the result—the sinus healed firmly, the pain and throbbing disappeared, and the patient is now able to walk six to eight miles at a time without inconvenience or bad result of any kind. Occasionally a slight discharge comes from the canal which now leads into the neck of the femur.

Case VIII.—*Acute Osteitis at Upper End of Femur; Deep Trephining; Cure.*—Mr. H. O'R., aged thirty years, came under my care on the 24th of February, 1887, giving the following history:—A fortnight before he came under my care he had fallen on his right hip. At the time he experienced rather sharp pain, but after resting for half an hour this subsided, and he was able to walk a distance of two miles. In some hours pain, stiffness, and swelling set in, and on the following night he became feverish. Leeches, fomentation, and poultices were applied, but pain continued. Extension was tried, but afforded no relief. Shiverings occurred on the fifth or sixth day, and when I saw him his condition was as follows:—Pulse, 120; temp., 103·2°; hectic appearance; complained of shiverings; feet clammy; tongue coated; urine scanty, high coloured, and depositing large amounts of lithates. Any attempt at moving the hip caused great pain. There was considerable swelling round and over the great trochanter, superficial œdema in upper third of thigh, and a sense of deep fluctuation in the trochanteric region. Thickening of the upper third of femur could be distinctly made out, but the patient explained that this state of things existed since he was seventeen years old, when he had a severe attack of inflammation of the hip, which confined him to his bed for some months.

Fig. 5.

Operation.—Free incision down to bone; trephine, 16 mm., passed one half inch into bone, removing piece; discharge of sero-sanguineous fluid; reintroduced and passed to a depth of one and a half inches in the direction shown in Figs. 6 and 7, giving exit to some pus. The scoop removed necrosed cancellous tissue, and cheesy deposit, and there was free hæmorrhage. Fig. 5 is a representation of the cervix femoris, which I trephined before conducting this case, and shows the course which the trephine took in the operation.

The after-treatment was carried out as in other cases, the patient making a permanent recovery, although at intervals of three or four weeks small abscesses burst through the cicatrix. For the past eight months there has been no discharge, nor has the patient had any uneasiness in the hip.

Case IX.—*Chronic Tubercular Osteitis at Upper End of Femur; Small-trephine Opening and Drainage; very slow Recovery.*—Miss H. F., aged twenty years, came under my care in December, 1887, with symptoms as in Case IV. Operation, January, 1888, with 8 mm. trephine. For weeks after small spicula of bone and curdy pus came away, and for months free suppuration was going on, notwithstanding constitutional and local treatment carefully carried out. In May I enlarged the opening in the bone, scraped the cavity, and dressed as in other cases. Even after this, firm healing was not established for three months.

The above cases illustrate the procedure and after-treatment sufficiently, so that I may conclude the list of trephining by merely referring to the position and number of bones which I have trephined during the past few years. In addition to the cases noted above, I have trephined the lower end of femur at the inner condyle in two cases; the upper end of tibia in three; upper end of humerus, two; olecranon, three; lower end of radius, one; and astragulus in one.

Useful limbs resulted in all the cases in which the operation was conducted early—the only failures being in one case of osteo-arthritis at the ankle; Symes's amputation became necessary. In one shoulder case resection was demanded; in one case of trephining the olecranon ankylosis occurred at the elbow. In one of the hip cases there is still a slight discharge, but no pain or inconvenience;

Fig. 6.

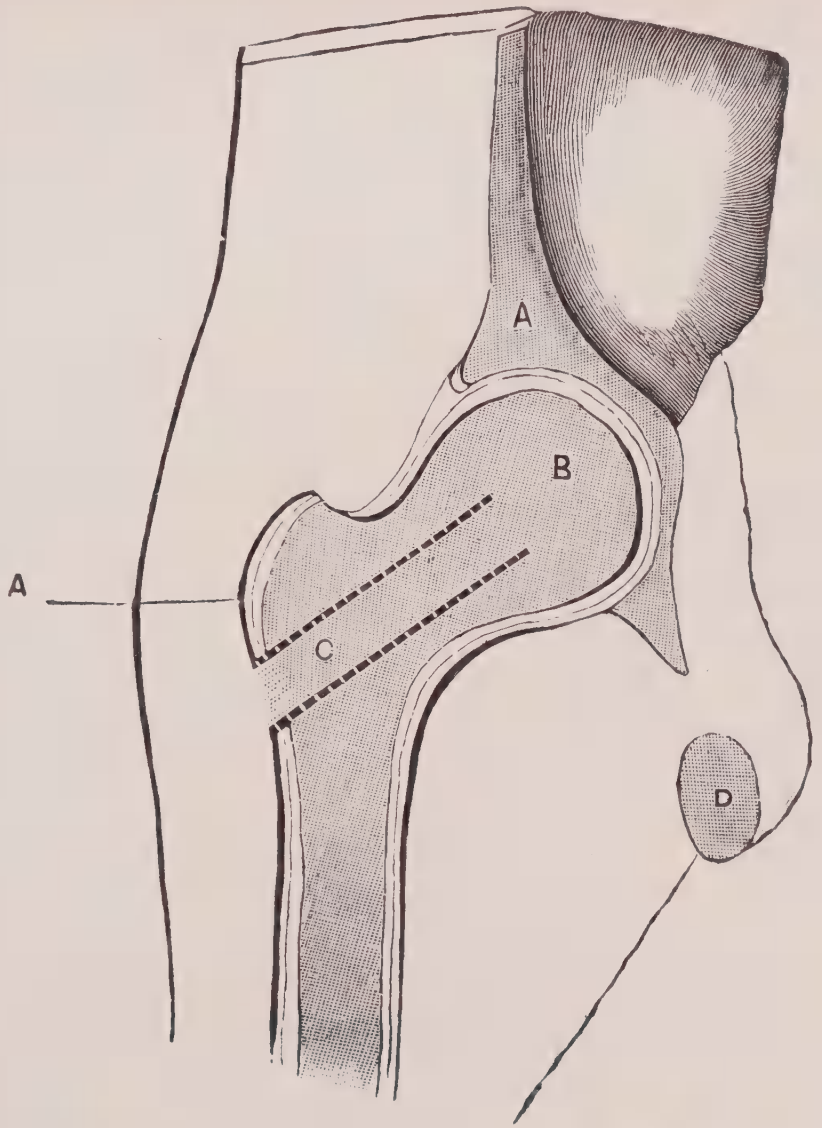


Fig. 7.

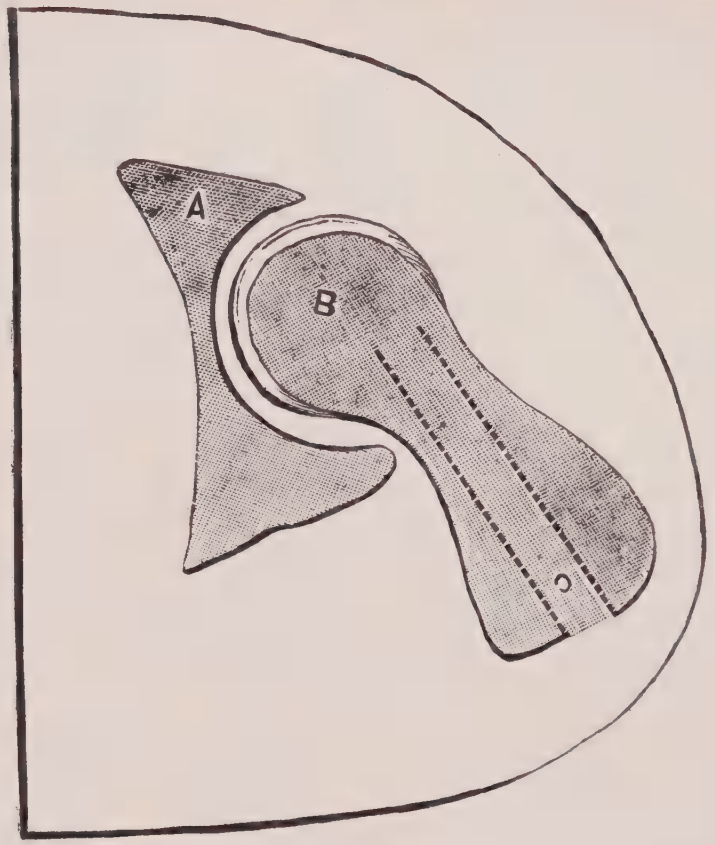
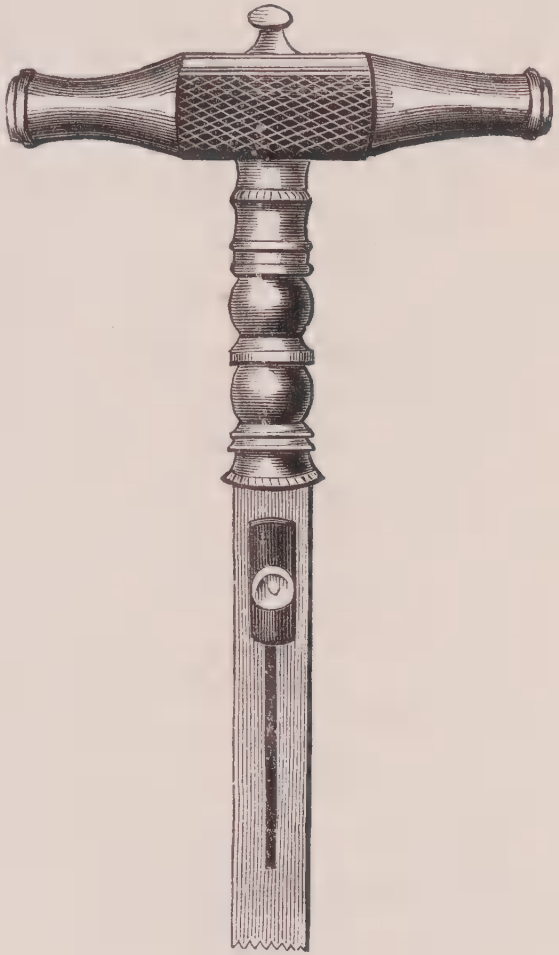


Fig. 6.—Lateral vertical section through hip.
B, head of femur ; C, course of trephine ; D, os pubis.

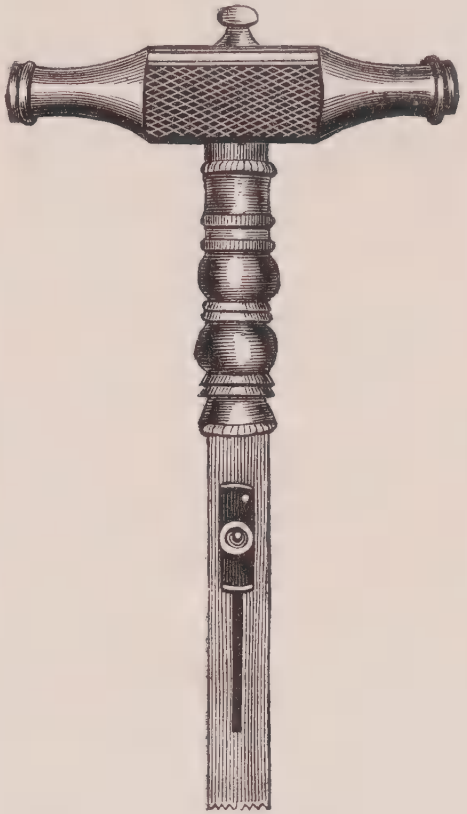
Fig. 7.—Horizontal section through hip-joint.
A, pubic portion of acetabulum ; B, head of femur ; C, course of trephine.

Fig. 8 *a*.



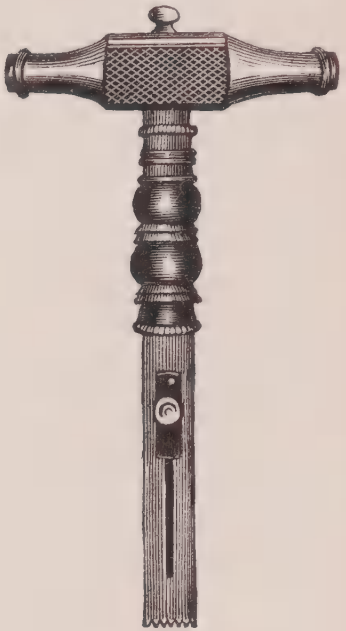
16 mm.

Fig. 8 *b*.



12 mm.

Fig. 8 *c*.



8 mm.

MR. M'ARDLE ON TUBERCULOSIS OF BONE.

Trephines for use in deep-seated Bone Disease, reduced one-half.

while in the last case noted, although she is well, sufficient time has not elapsed since healing to justify me in claiming the case as a success. One must be prepared for abscess formation on and off during the months following trephining unless the diseased tissue is thoroughly eradicated; but when an abscess forms in the bone the trephine wound, filled with soft tissue, offers an easy exit for the pus, and perforation of the articular cartilage will not be likely to occur.

The cases noted show the method of treatment which I believe to be most generally successful in dealing with osteal tuberculosis; and in calling attention to the advantages of trephining I am anxious to point out the situation in which the trephine can be most satisfactorily applied to different bones, and the conditions which must be fulfilled if we expect to obtain the more beneficial effects of the operation.

Beginning with the upper extremity. The head of the humerus can be reached and readily drained by an incision along the bicipital groove, drawing outward of the deltoid, and slight rotation inwards of the arm. The periosteum is then raised along the outer side of the biceps tendon, avoiding its sheath. The medium-sized trephine, 12 mm., for adults, the smallest one, 8 mm., for children, can be applied so as to pass obliquely from below the greater tuberosity upwards and inwards through the epiphysary cartilage into the head. This allows (when the arm is placed across the chest) free drainage so long as the patient sits up or lies on the opposite side. When the upper end of ulna is diseased in adults the crown of the trephine should be placed, not on the most prominent point of the bone, but over a spot $1\frac{1}{2}$ to 2 cm. from the tip of the olecranon, the direction of the channel being upwards and forwards. Here the small trephine usually suffices, even in adults.

At the wrist the only bone I have trephined is the radius, and in opening it I believe the operation should be performed on the outer and anterior aspect 2 cm. above the styloid process. This allows free drainage when the arm is placed in a sling in the prone position, and prevents undue interference with the dorsal or flexor tendon sheaths.

My idea of trephining for tubercular disease of the upper end of the femur is that we should at once reach the diseased centres, that the channel leading thereto should not be tortuous, and that its direction should be such as to favour drainage. If the trephine does not reach the cheesy deposit or intra-osseous abscess, gouging, scooping, or other secondary operation is necessary to remove the diseased tissue. The mere opening of the bone does no good, and if we are dealing with advanced tubercular osteitis, on the contrary, it allows (unless extreme care is taken with the dressings) a simple to become a septic inflammatory process, or it excites still further the tubercular disease, and brings about surgical dissemination of the tuberculosis which, becoming general, ends in the production of pulmonary, hepatic, renal, or other fatal form of the affection. Should we, after a superficial application of the trephine, proceed to scoop out the diseased cancellous tissue of the upper end of the femur, sacculation is almost certain to result and form a serious obstacle to the complete and essential drainage of the bone, unless the opening be large and in good position. In adults the large trephine (16 mm.) will be necessary; in children under ten the small one (8 mm.) usually suffices. The point of application, in the absence of any softening of the bone surface, is, in adults, $1\frac{1}{2}$ cm. below the most prominent part of the great trochanter, and a little behind the middle line of the bone; in children, the central point of the great trochanter answers. The direction in adults is upwards and inwards towards the middle of Poupart's ligament, as in Fig. 6, C, inclining the point of the trephine forwards, as in Fig. 7, C. In children the direction is similar, but care must be taken not to needlessly interfere with the epiphysary cartilage. This can be done by withdrawing the instrument after it has passed through 1 cm. of bone, the inner surface of which should be examined for pus, cheesy deposit, or other evidence of disease. If none shows, and there is undoubted femoral disease, the trephine may be reintroduced and a portion of the cancellous tissue removed until a clear channel is made reaching up to the diseased spot.

Macnamara, in his work on "Diseases of Bone," says:—"In cases of osteitis of the head of the femur I have not been able to

A SUBCHONDRIAL. CENTRE.

B B INTRAOSSEOUS.

C SUBPERIOSTEAL.

DR. McARDLE ON TUBERCULOSIS OF BONE.

reach the diseased bone with a trephine, but in its condyles, the head of the tibia, and so on, if a portion of bone can be removed in this way from the epiphysial cartilage, the result is often most satisfactory."

What the ordinary instrument does fairly in the superficial bones the long-barrelled trephine which I have had specially constructed does in the upper end of the femur and humerus—that is, it removes diseased bone lying subjacent to the epiphysial cartilage and even from beneath the articular cartilage. The set of trephines, shown in Fig. 8, I exhibited some time since when bringing some of the cases operated on before this Section of the Academy. The sizes, which I have found suit admirably, are 16, 12, and 8 mm. Less than 8 mm. will not give free exit to discharges, and many of the failures following paracentesis ossium may, I think, be attributed to an insufficient opening in the bone.

Plate I. shows the situation in which femoral tuberculosis usually begins; as in other joints, the subchondrial (*a*), intra-osseous (*b b*), and subperiosteal (*c*) are represented, the one most frequently met with being that near the epiphysary line. If a deposit here can be removed, or an osteitis be checked before destruction of the cartilage occurs, a joint with normal function should result.

Section C.—Joint Tuberculosis.

The next class of cases to which I would refer are those where the local disease engages intra-articular structures, or structures other than bone, so intimately connected with the synovial membrane that extension thereto of disease is likely to take place, unless prevented by judicious treatment. The study of the ultimate results of resection for tubercular disease, especially if advanced, is very disheartening, and this holds good for all the joints. If we can prevent the spread of disease in an early stage, there is a fair chance of thoroughly eradicating it; but, if we allow a circumscribed centre to infect a neighbouring joint, fungous synovitis, osteo-myelitis, or other serious trouble results, the outcome of which is often destruction of the joint, and not rarely a general infection. Bidder, in his paper on Joint Tuberculosis (*Deut. Zeit. für Chirurgie*,

p. 80, 1884), gives at length thirty cases of scrofulous disease of the larger joints, the details of which have a direct and important bearing on the question of active surgical treatment of local tuberculosis, since his observation extends over many years, various methods of treatment were fairly tried, and the patients were watched carefully for long periods. As a result of his experience, he recommends early removal of diseased tissues by arthrectomy or resection, as the case may require. In the former operation he prefers opening joints by flaps rather than simple incision, as the erasion can be more complete. He condemns in unequivocal terms the palliative treatment, and shows by his records that it has nothing to recommend it. In looking over Dr. Berthold Körff's paper, "Uber die Endresultate von Gelenkresection" (*Deut Zeit für Chirurgie*, p. 149, 1885), I find that the cause of death in most of the fatal cases was meningitis, phthisis, amyloid liver—in other words, general tuberculosis arising from a local centre which had been allowed to remain long untouched. We see that early resection is followed by rapid healing of the wound and restoration of the patient to health, while the ultimate results are infinitely better than when delay has occurred. If the early removal of disease, even when it has engaged large and vascular structures, is followed by good results, the removal of localised centres should still further reduce the immediate, as well as the ultimate mortality mortality. The following are cases on which I operated with that object.

Case I.—Patrick L., aged thirty-eight years, a farmer, came under my care on October 10th, 1883. For eight months he had suffered much with severe pain in the centre of knee, swelling and stiffness of joint. To relieve these symptoms blisters were applied, and afterwards the limb was put up in a plaster splint and allowed to remain therein for three months, pain during all this time being more or less marked. When the plaster was removed he found his leg stiff, and swelling had slightly disappeared, but every attempt at raising the limb caused pain.

Condition on Admission to St. Vincent's.—He was unable to get about on the leg without great pain, and so his general health had become impaired. The leg was slightly flexed at the knee, the calf and thigh wasted, the foot cold and clammy. Above the

patella a fluctuating swelling extended four inches, while below and on each side of that bone, and engaging the sides of its ligament, was a doughy swelling, pitting deeply on pressure; deep-seated fluctuation could be detected in this mass, and the pain was chiefly referred to a point deeply placed in the joint behind the ligament. Believing that the synovitis in this case was kept up by suppurating tubercular material behind the patellar ligament, I performed the following operation:—An incision was made as for resection of the knee, extending from one condyle to the other; the skin, fascia, and tibio-patellar ligaments were cut through on both sides; at this stage it could be seen that gray granulation tissue had invaded the entire region behind the ligamentum patellæ. The fungous tissue projecting on each side was cut through and exit given to a quantity of curdy pus; the fungous tissue was then scraped away, the cavity mopped out with chloride of zinc solution, a large tube passed behind the ligament, and antiseptic dressings applied. First dressing 24 hours after operation; discharge copious and tinged with blood; temperature, 99·2°. Second dressing three days later; slight discharge of creamy pus; swelling much less. Third dressing seven days later; wound granulating; very little discharge; supra-patellar swelling quite gone; no pain or uneasiness in joint.

After this the patient's strength improved rapidly, and three months after the operation he had a perfectly movable and painless joint. Since that time he has continued to work at his farm, and he has never suffered any inconvenience from the knee.

Case II.—Mary H., aged eighteen years, general servant, came under my care on the 6th of January, 1885, with symptoms resembling those noted in Case I. The history of deep central pain, stiffness, and swelling, unrelieved by blistering, firing, and leeching, caused me to examine the infra-patellar region carefully, and I found that a deep nodule could be felt at the most prominent part of the swelling, which appeared to the inner side of the patellar ligament. This nodule was tender on pressure, and when patient attempted to raise her leg, severe pain occurred at this part.

Operation.—I made an incision from the inner condyle to the front of the ligamentum patellæ, and on reaching the painful nodule found it to be as in Fig. 6, Plate II.—a cheesy mass, surrounded by inflamed and indurated tissue. I dissected out the mass with the handle of a scalpel, and discovered at its outer side a small collection

of creamy pus. The wound was washed out with carbolic solution (1—100), a drainage tube laid in, three sutures placed at the inner angle of the wound, and antiseptic dressings applied.

This patient made an uninterrupted recovery, and in one month she left the Convalescent Home at Linden with a thoroughly useful limb.

Case III.—Peter D., came under my care on the 17th of September, 1885. For three years he had suffered a great deal from pain and swelling in the left knee, arising from inflammation, the result of a kick from a horse. Ordinary methods of treatment gave him no relief, and so his leg was placed on a straight splint, and morphin administered, until after two months' rest, and continued drugging with morphin, he obtained some relief. He was then brought to the seaside for four months, still wearing the splint. Here his strength improved rapidly, and when leaving for home he had little or no pain. After wearing the splint for seven months, he tried to get along without it, but found that he could not raise the limb without severe pain radiating from the middle of the joint.

When he came under my care, I found that firm fibrous union had occurred between the femur and tibia, and the patella and its ligament were pushed forward by an elastic swelling, which was painful on pressure below the bone, and gave a sense of deep fluctuation. In this case I thought it advisable to leave the limb fixed, as it was in good position, and so I performed the following—

Operation.—An incision was made from condyle to condyle down to, but, as in the other cases, not through the ligament. The lateral portions of the incision were carried into the infra-and post-patellar tissues, and exit was given to a considerable amount of creamy pus, and masses of granulation tissue were removed by scooping. On the inner aspect of the joint a large patch of the synovial membrane was removed, and sero-synovial fluid discharged. The outer part of the synovial membrane being healthy, it was not interfered with. A large drainage tube was placed in the pouch above patella, gauze plugged well into the wound, and antiseptic dressings applied. The temperature never reached 100° F., nor was there a single unfavourable symptom until healing was complete. The knee was allowed to remain fixed, being supported by a posterior splint. One month after operation, he was able to get about with the aid of a stick. Three months later he returned

to the country, and since then he has been in good health, and can use the limb as freely as the other. There is now bony ankylosis, and the limb is very slightly flexed. If before applying the fixed splint the infra-patellar deposit had been removed, it is more than likely a movable joint would have been the result.

If such cases as the foregoing are allowed to pass as cases of chronic or tubercular synovitis, the damage to the joint can easily be foretold; for, although many cases of deep chronic abscess of the knee recover without operation—and we may argue that they arose from tubercular deposit behind the ligament—the vast majority will, in the absence of operation, lead to destructive lesions of the joint. The usual course of such cases is—the patient first finds that kneeling, sudden flexion of the leg on the thigh, or any such effort, causes pain deep in the joint. After a little time stiffness occurs and swelling, which will be, for the most part, due to distension of the synovial membrane, as the result of a subacute synovitis arising from the presence of the tubercular lesion in the infra-patellar pad. The synovitis is the trouble which causes the patient to seek surgical advice, and for the synovitis he is treated. Meanwhile the original disease spreads in all directions, extending into the synovial membrane, which becomes thickened; the fungous growths invade the ligaments, and destruction of the joint is the result.

Besides the above cases, I have removed well-defined masses of tubercle from the following situations:—In a fourth case from behind ligamentum patellæ; in one from popliteal space; one from inner side of knee, where it had infiltrated the lateral ligament; in two from the back of wrist, infiltrating posterior ligament, and causing synovitis; in one from front of elbow; one from calcaneo-astragaloid joint, and in one from the metacarpo-phalangeal joint of index finger. Of these ankylosis remained in one knee case and in one wrist; in both it was present before operation.

In cases other than the above which have come under my notice, the following conditions, illustrated in Plate II., were noted:—

Case I.—*Resection of Knee.*—Bones healthy; infra-patellar pad enlarged, infiltrated with tubercular matter, adherent to the con-

dyles of femur, as in Fig. 1, thus preventing the action of the quadriceps muscle. The lower edge of the patellar cartilage was raised by a subchondrial extension of the tubercular tissue.

Case II.—*Resection of Knee.*—Infra-patellar pad and bursa the seat of extensive tubercular infiltration, as shown in Fig. 2, bulging into joint and invading lateral ligaments. Inflammatory exudation raising cartilage from outer condyle.

Case III.—*Arthrectomy.*—Extensive cheesy deposit in infra-patellar pad; bursa inflamed but not tubercular; semilunar cartilages invaded by disease; edges frayed and softened, as in Fig. 3. In this case removal of the cartilages and infra-patellar pad secured a good limb, with a fair amount of motion, but not so useful as early operation would have left.

Case IV.—*Resection of Knee.*—Fungous granulation tissue spreading from infra-patellar pad, destroying semilunar cartilages and invading lateral and crucial ligaments, producing subluxation of knee, as in Fig. 4. In this case the disease evidently commenced in the bursa, behind the patellar ligament, as here cheesy deposit was marked, while elsewhere there were only scattered little specs of cheesy material in the granulation tissue. The result was a good fixed limb, but recognition of the disease at the start might have allowed of a good joint being preserved, as the bones were perfectly safe.

Case V.—*Resection of Knee.*—The disease in this case commenced in the bursa, spread to the fatty tissue, and invading the internal semilunar cartilage, caused erosion of the upper surface of the tibia, as in Fig. 5. Resection was followed by a useful limb, although sinuses continued to discharge for some time.

Several cases which I have examined, and which seemed to me as having started in the infra-patellar pad, I omit, as, not having definite proof of the course taken by the disease, I could not set them down in the above list. In reference to early operative interference in cases of primary tuberculosis such as these, all I can definitely state is that synovitis is cured, extension of the disease prevented, and a sound movable limb produced. Recurrence may take place, but that is unfortunately possible after the most extensive operation.

EXPLANATION OF PLATE II.

In all the figures A represents the synovial sac; B, the patella; C, fungous granulation tissue; D, cheesy deposit.

In Fig. 3, E and F represent the external and internal semilunar cartilages; G, the crucial ligaments; and H, the patellar ligament.

Fig 1

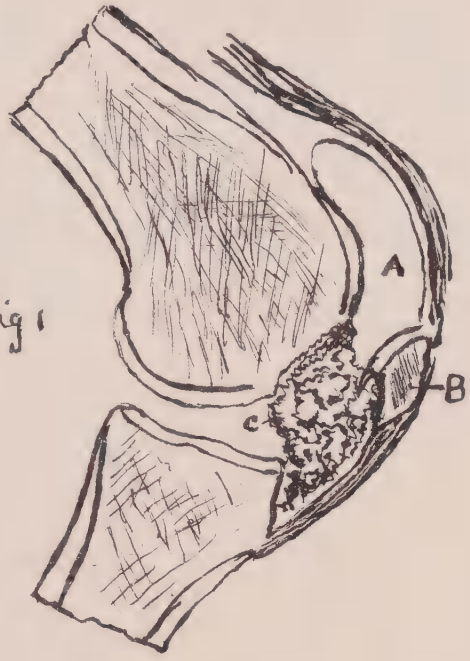


Fig 2

Fig 3

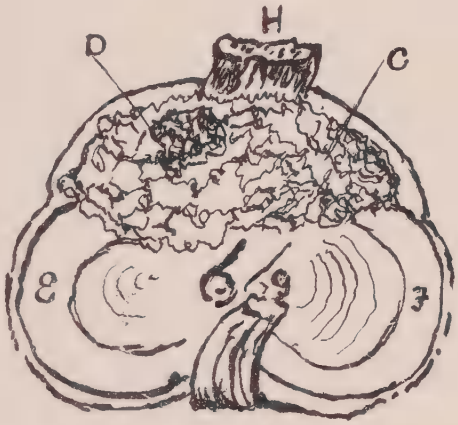


Fig 4

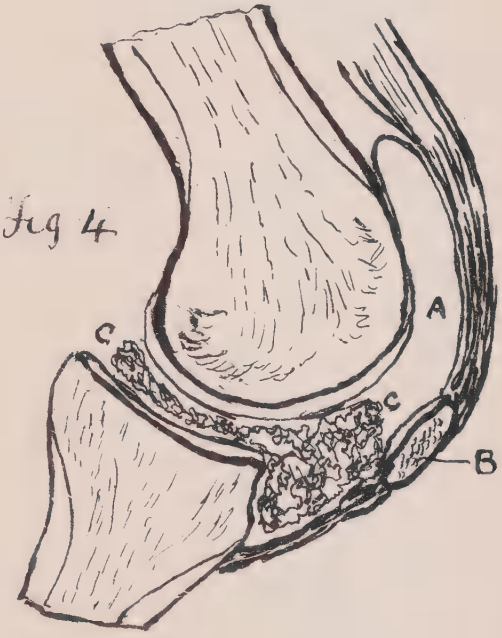


Fig 5

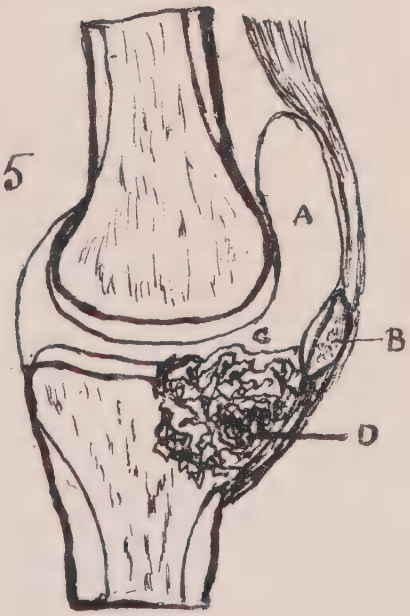
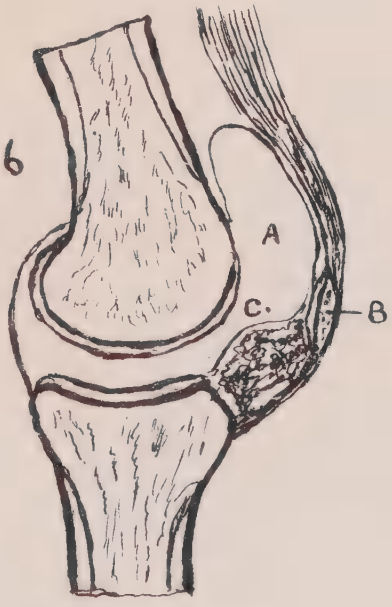


Fig 6



In reference to these cases of arthrotomy, the rules which I laid down for myself when undertaking them were :—

1st. Make the incision or incisions in such position that arthrectomy can be carried out if necessary, or even complete resection if required.

2nd. The openings should be sufficiently free to allow of complete removal of diseased structures.

3rd. The incision should be in proper position to allow of free drainage.

Neglect of any of these precautions will lead to much delay, incomplete removal of diseased tissue, or generalisation of the tuberculosis. On this head Ollier says :—"The danger in such cases does not arise from opening the joints too much; it comes from not opening them sufficiently. The openings must be made sufficiently long and numerous, so that not a drop of septic liquid can accumulate in the cul-de-sac articulaire."

Arthrectomy, which some think should take the place of resection, unless in advanced bone disease, has not the support lent by numerous good results. Many of the recorded cases show that this operation should be limited to cases in which the disease is very well defined and bone disease, except superficial erosions, absent. Instead of setting ourselves the profitless task (as writers are now doing) of trying to prove that arthrectomy, as against resection, should be the more general operation, we can best advance surgery and benefit our patients by studying—

1st. In what joints can erasion be carried out with success.

2nd. In what cases and at what stages is it indicated.

3rd. What conditions render resection necessary.

This done, and when individual operators have experience of many operations of both classes, we may answer the question, How do the operations stand when compared as to—

1st. The danger of the procedure?

2nd. The immediate result?

3rd. The chances of local return or general dissemination of the disease?

4th. The immediate and future functional activity of the limb?

At present surgical literature does not contain sufficient material on which to base a truthful answer to these questions.

If my scanty record be a contribution, however small, to the elucidation of this question, I hope I shall not have occupied your valuable time in vain.

ON STRICTURE OF THE URETHRA AND SOME OF ITS COMPLICATIONS.

By R. F. TOBIN, F.R.C.S.;
Surgeon to St. Vincent's Hospital; late Assistant Professor of Surgery, Army Medical School.

[Read in the Section of Surgery, January 18, 1889.]

A SUBJECT seems to me to require an apology on account of its triteness only in cases where trituration has resulted in uniformity of opinion. As such is not the case on many points concerning stricture of the urethra, I ask you to bear with me while I lay before you some remarks founded on my observation of that disease. Should these appear to you disjointed and incomplete, let me say, in extenuation, that this must often be the case when one does not allow one's imagination to fill up the interstices. In a very able paper read before this Section in 1885, Mr. Thornley Stoker gave it as his opinion, that at that time Dublin surgeons were pretty well agreed—1st, on "the superiority of internal urethrotomy over any form of dilatation;" 2ndly, on the advantage of the method of Maisonneuve over that of Civiale and his followers; and, as far as I can ascertain, these statements continue to be a fair expression of the opinions of to-day. For myself, I am inclined to give to them a full and unhesitating assent. But if we look outside Ireland we shall find some of the most experienced authorities giving a direct contradiction to both assertions.

Of the first, directly the reverse is maintained by Mr. Reginald Harrison; of the second, by Sir Henry Thompson. In the presence of such opposition, it is surely right to continue to look well into the matter, to carefully record our experiences, and publish them from time to time, till we either arrive at agreement or at an understanding of why we differ.

While I was in the army a very large number of cases of stricture came under my care, especially during my last six years, when I was in charge of the surgical side of the Royal Victoria Hospital at Netley. As every soldier who is invalided from abroad has to pass through that hospital, taking there with him a medical history sheet, in which has been recorded every incident affecting his health, it happens that there are there facilities that do not exist in civil practice of studying on a large scale the origin and progress of disease. The opinion that I formed, under these circumstances, of the cause of urethral stricture was this—that in most of the severe and intractable cases syphilis as well as gonorrhœa is a very important factor. Of course, the majority of medical history sheets contained no entry of secondary syphilitic manifestations, yet such a record was seldom absent when a hard unyielding stricture existed. Gonorrhœa seemed to start the disease, and syphilis to fix it. It started it, as a rule, in that part of the urethra, in which, owing to its shape and horizontal position, discharges are inclined to lodge, decompose and irritate. Soldiers who had suffered much from gonorrhœa seemed to be aware of this pathological fact. I have often seen a man, when asked if his gonorrhœa had ceased, and when no discharge appeared on squeezing the anterior portion of the urethra, make sure of his condition by passing his hand between his legs and making pressure on the perinæum from behind forwards.

As to the treatment of stricture I believe the medical history of the army would furnish evidence very unfavourable to that by gradual dilatation, and this notwithstanding that time is not as much an object as it is in civil life, and that army medical officers are, as a rule, well skilled in the passing of instruments. For myself, experience caused me to discard in advanced cases every form of dilatation in favour of internal urethrotomy; and, curious to say, for the very reasons that seem to have induced Mr. Harrison to discard internal urethrotomy in favour of dilatation. I have found the results of urethrotomy more complete and more lasting than any other form of treatment, and the operation followed by less urethral fever than is the operation of divulsion. The worst

attacks of urethral fever noticed by me have followed treatment by what I may call rapid gradual dilatation—*i.e.*, in cases where, finding a stricture in good humour, I have been induced to dilate it four or five sizes at one sitting by passing as many sounds. The little permanent good effected by such treatment, and the severity of the fever often induced thereby, make me now look on this practice as highly objectionable. To Holt's and other dilators of that class I would give second place as febrile excitants. Perhaps want of skill in their use may account for that opinion. I say this because I know that, as I go on using the urethrotome, this particular and peculiar form of fever to which I am referring seems to recede further and further from me. Whether this is due to increased attention to the aseptic condition of instruments, to the non-retention of a catheter in the bladder, to the use of cocaine as a local anæsthetic, to increased acquaintance with the method of performing the operation, or to all four causes combined, I cannot say, for it has followed in the wake of all four.

On hæmorrhage as a complication of internal urethrotomy I do not look with much dread. Only in two cases did it give me trouble, and that not to an extent to cause anxiety.

On the method of performing the operation I hold the following views :—For brevity sake I express them dogmatically, but I hope you will look on them as queries put to you to elicit information on points on which there is not universal agreement. Twenty minims of a 5 per cent. solution of cocaine deposited in the urethra, in the immediate neighbourhood of the stricture, is, as a rule, a sufficient anæsthetic. The operation is better done with a straight instrument than with the curved one in ordinary use. Teevan's urethrotome finished where it begins to curve upwards makes the instrument I refer to. In such shape it is handier, and can be made to cut in every direction with more facility. It should be invariably used with the fine flexible guide attached. Such a precaution prevents the point of the instrument injuring the walls of the urethra, and secures it against entering a false passage. Of course one does not proceed with the operation unless confident that the guide has reached the bladder. I hold that one is never in doubt on this

point. When the guide has entered the bladder the feel is so characteristic that the sense of touch may be relied on as completely as would be the sense of sight. When dividing the stricture the blade should be turned towards the nearest urethral wall; when the position of the opening in the stricture cannot be ascertained, or when it is central, the cut should be made in the roof of the passage. In other words, in making your incision, get as far away as you can from any mass of cicatricial tissue. This is, I think, in accordance with the practice of surgery in other parts, for when liberating a cicatrix on the surface of the body we do so if possible by cutting through sound tissue rather than through the cicatrix itself. It is, of course, not always possible to ascertain at which side of the urethra the obstacle to be overcome is chiefly situated, but if an angular bend is given to the top of a fine exploring bougie, and it is passed down to the obstruction it will often be found that when the point is turned one way it will pass with ease, while when turned the other it impinges against something, thereby defining the relation of parts. The stricture before being incised should be well stretched over the blunt shield of the knife, and then if possible cut completely through by one deep cut. After division it should at once be stretched by the passage of an instrument of not less size than "14 English," and it should then be left alone for at least three days. If at this period the incision could be seen, I believe, from observations made on the dead and living subject, in cases of narrow stricture, that in consequence of the stretching it would be found to have undergone the change that does a short slit in an india-rubber tube when its walls are drawn latterly aside, that is, from a longitudinal to a transverse direction, and I believe that the subsequent healing is rather by the drawing together of the antero-posterior ends of the wound than by the approximation of the lateral edges.

As to the complications of stricture there are but two to which I wish to make allusion. It has been so impressed on students that false passages when they exist will be found in the floor of the urethra that beginners now expend all their energies on its roof. Some teachers have such an emphatic way with them that in driving

in one truth they drive out another. A false passage in the roof, however, has this of excellence about it, that if the man who makes it is gifted with perseverance, neither he nor his patient may know anything about it. For since the urethra curves upwards just behind the usual seat of stricture, and is, moreover, as a rule distended, there is every chance that if the instrument is pushed on it will find its way back into the normal passage, and so on to the bladder. In fact, just as an instrument wanders into the rectum when it has made a false passage in the floor of the urethra, so when its point of departure is in the roof it makes its way to the distended urethral pouch that is waiting for it behind the stricture.

The only two cases of stricture that I had an opportunity of dissecting last year at St. Vincent's Hospital gave evidence of this occurrence. In each there was a dissecting false passage situated above and acting for a strictured portion of the urethra. In cases where catheterisation detects this state I should say that opening the urethra into the false passage or the false passage into the urethra by means of internal urethrotomy, would yield excellent results. The other complication to which I wish to refer is also the result of the misuse of instruments. After thoroughly washing out the bladder in cases of cystitis due to stricture I have often wondered at finding in a very few minutes pus-discoloured urine, perhaps more discoloured and more offensive than what had come before, flowing through the catheter. In some cases no doubt this is due to the state of the kidneys and ureters; but in a case that died last year in St. Vincent's Hospital, I discovered another cause for the symptoms, and one that may have existed in others, for it would easily escape notice. Between the walls of the bladder, which was sacculated, and the recto-vesical fold of peritoneum there was a cavity capable of containing some six ounces of fluid. This communicated with the bladder by a small opening through the bottom of one of the sacculi, and was no doubt produced by some operator as he triumphantly drove home a metal instrument to satisfy himself and others that he was well in the bladder. Whoever studies the anatomy of the parts in question in a frozen section cut with the bladder collapsed and the rectum distended—

a condition that may often exist when instruments are being passed with a view to gradual dilatation—will see how guarded he should be in applying that plan of treatment to a patient with a sacculated bladder.

In conclusion, in order to keep up the disconnected character of my remarks as well as to illustrate what may be done by patients who have been taught to pass instruments for themselves, let me narrate to you the story of a soldier who was so instructed, as the eccentricity of the individual renders it interesting apart from its clinical aspects:—Private M'C., aged forty-two, was admitted into Netley Hospital in January, 1880, and gave the following history of his ailment. He had suffered from a difficulty in making water nearly all his service, and the regimental doctor—who, he said, "was tired of passing instruments for him"—had taught him to pass one for himself. "Well," he continued, "one night about a year ago while making my way back to barracks I found I wanted to make water and could not, and my instrument was away in my box. As I never could wait till I got to it I rubbed the snow off a stone at the side of the road and sat down on it. I then took the wire off a soda-water bottle I had with me and made it into the shape of a catheter, bending back the tip so that it would not be too sharp. I passed this all right and made water, but when I came to draw it out it broke off at a kink, and the greater part remained in my passage. I did not report sick for three days and then it was to a civilian doctor who was attending the troops. Well, he got a forceps and tried repeatedly to get it out, and when he could not, says he to himself, I will put it where no one else can, and with that he shoved it back into my bladder." The patient who told the above story was a haggard, broken-down man, who had evidently gone through much suffering, and who had a great dread of being touched. On that account, and as I saw it was a case for operation, I deferred making an examination of him till I could do so in consultation with my colleagues. When we had assembled for that purpose next day we found that M'C. had barricaded himself in the lavatory of the ward in true Irish fashion, determined to stand a siege rather than any surgical inter-

ference. A breakdown in his commissariat, however, caused him to surrender before supper-hour. As it never would have done to have allowed, through an incident of that sort, an idea to circulate that men were being operated on against their will, I told M'C. so—adding, "and to further contradict your action I will not operate on you till you go down on your bare knees and raise up your hands and pray of me to do so." Time went on, other patients left cured, but of course there was no change in M'C.'s condition. At length, on going one morning into the ward in which he was located, I was met by a spectacle consequent on his taking my words literally—he was on his bare knees with his hands lifted up praying for an operation. A few days later I removed the wire I now hand round through a median incision in the perinæum. You see that it is about seven inches long. During its stay in the bladder it had got covered with phosphates till the collection was almost of the size and shape of the soda-water bottle to which the wire originally belonged. The mass after being broken up was easily removed by means of forceps, scoops, and irrigation. The patient made a good recovery.

TREATMENT OF TUBERCULAR PERITONITIS BY ABDOMINAL SECTION AND FLUSHING OUT, WITHOUT DRAINAGE.

By ROBERT O'CALLAGHAN, F.R.C.S.;
Fellow, British Gynæcological Society ; Surgeon, Carlow Infirmary.

[Read in the Section of Surgery, Feb. 22, 1889.]

The treatment of tubercular and suppurative peritonitis has within a comparatively short period been rescued from the expectant and practically impotent treatment of the physician, and has now passed into the yearly-increasing domain of the surgeon, and it is not the least of the modern triumphs of our art.

In England and abroad this is a universally accepted fact, but, from statistics, we do not appear to have followed in the footsteps of progress as regards this treatment, and we seem slow in accepting the fact that the principles applied to general surgery are just as applicable to that of the abdomen. It is with a hope that a stimulus may be given in this direction by this great *post-graduate* teaching centre that I venture to bring before your notice this evening my small but gratifying experience, and, as far as I can collect them, the statistics of this treatment up to the present date. In a great many recorded cases the abdomen has been opened under an erroneous diagnosis for ovarian and other tumours, and in exploratory incision tubercular disease has been discovered, the abdomen cleansed and closed, and the patients have done well. I wish, however, to refer particularly to-night to those cases in which this degeneration of the peritoneum has been diagnosed and abdominal section with flushing out of the cavity has been adopted with complete success.

Before going further I will, with your permission, give you a

brief history of my cases, and I am not aware of any such being reported hitherto in Ireland:—

Case I.—James G., aged fifteen, whom I had the pleasure of showing to you at the last meeting of this Section, was admitted to the County Carlow Infirmary, June 7th, 1889. Family history was good, all living, save his mother, who died of fever. He presented the following appearance:—General emaciation, pale face, dark circles round the eyes, with bright malar flush and hurried respiration; a large, distended abdomen, very tumid, yet symmetrical. He complained of dull, colicky pains, which were never severe, but came and went every few hours, night and day; more or less tenderness on pressure; appetite completely gone; could only take small amount of fluids, which he frequently vomited; constipation; secretion of urine scanty; skin dry and harsh. He had been suffering in this way for a month previously to his admission, and traced his illness to a blow on the abdomen from a boy's head while playing football. He was sent in to me as a case of hepatic dropsy, and at the time, thinking it might be so, I treated him for hepatic mischief, and also painted the abdomen night and morning with iodine. He, however, got worse daily, and diarrhœa set in. Taking the facts of the case, the age of the patient, the condition of the main organs, I decided in the first week of admission that it was a case of tubercular disease, and that the only hope left to the boy was operative interference. In this diagnosis his former physician, Dr. W. O'Meara, concurred, and kindly gave ether for me, the pulse at the time being 98, weak and thready; temperature varied from 100° to 101°. I opened the abdomen in the mesian line, midway between pubis and umbilicus, sufficient to admit my finger, and speedily emptied it of a gallon or more of a sero-purulent milky fluid. On everting the peritoneum I found it covered with little nodules like millet seeds, and on introducing my forefinger in every direction I felt the same state of affairs. With some little difficulty, owing to adhesions, I drew out a portion of mesentery, which looked like the "roe of a fish" or the "inside of a fig." The case seemed so hopeless I allowed Dr. O'Meara to examine this. I then flushed out the cavity with gallons of blood-warm water from the tap, meanwhile massaging the abdomen until the water came out quite clear. Then turning the boy on his side I emptied the cavity as much as possible, put two stitches in the wound, covered it with alembroth wool, and put him to bed.

With the exception of a slight rise in temperature on the third day, with sickness and tympanites, which was immediately relieved by a turpentine enema, followed by a seidlitz powder, he made an uninterrupted recovery. On the third week he was up, and had an enormous appetite, which he still possesses. You saw the other evening that there was no trace of disease left; and he is now about to join his brother in Australia.

Case II.—Bridget S., aged twelve, being an almost similar case, I will refer only to an incident worth mentioning—viz., that after five minutes' careful manipulation, "owing to extensive adhesions," I got into the peritoneal cavity, but unfortunately opened the bladder, which was adherent to within an inch of the umbilicus. Despite this serious accident she got quite well and strong under the same treatment. The wound in the bladder I stitched in the lower angle of the wound, as in cholecystotomy, and it was water-tight in the fourth week.

Case III.—Mary N. was a case of left lateral encysted peritonitis, at first thought to be by her medical attendant hydronephrosis. The right side, although full of tubercular glands and deposit, was quite separate by adhesions, which formed a large cyst. After the breaking up of all the adhesions, and the same treatment adopted as in the other cases, she is now quite strong, and the glands, which were quite palpable externally, have entirely disappeared.

I had two other cases of ovarian tumours complicated with this degeneration, one of which was successful. Of the fatal case I will give you a brief history of the *ante-mortem* state of the abdomen, which will illustrate to you the latter stage of tubercular degeneration in the abdomen.

When the case came to me there was a history of a swelling for fourteen months. Five months previously to my seeing her she was tapped for ascites, and evidently had an acute attack of localized peritonitis. The tumour—a multilocular one, which I showed last meeting here—was adherent in front throughout to a thickened and almost cartilaginous peritoneum. On removing it I found the liver atrophied and a mass of tubercle. The intestines, covered with miliary nodules, were bright red, and more like those of a fowl. The

mesentery was rolled round a portion of them, and the omentum, a firm mass with tubercle, "like a sausage." I did not flush out the abdomen in this case, for which error I was very sorry, as despite this state of affairs she lived three days with little or no discomfort.

These two last cases with others have led me to conclude that if any abdominal tumour is not removed within a reasonable time of its formation you are bound to have this form of degeneration set up as a complication, and, furthermore, illustrates the fact that more knowledge of the symptoms of abdominal disease in its early stage and that such is now curable, are wanted by the general practitioner, so as to prevent this repetition of criminal neglect, which, I am sorry to say, is too common—the surgeon not seeing them until they are past hope of any treatment, and in many cases they die unrecognised. The first successful case I find in which the disease was recognised before operation is one reported by Mr. Lawson Tait, in 1879—a parallel case to my own. Since then he has had a great many in all stages of the disease, with a mortality of 15 per cent. In the same year Dohrn operated on a child four years old, with success. Mr. Treves, in 1883, published several cases—one was in an infant sixteen weeks' old; it died three weeks after of acute miliary tuberculosis; and last month he reports many more cases. Clarke, of Huddersfield, published a successful case (*Lancet*, Nov. 5th, 1887). Kœnig gives four successful cases, in one of which he removed a bit of mesentery, in which he found the bacillus tuberculosis. Hofmokl published a case (*Wiener med. Woch.*, 1887). Poten, in the same year, gave a successful case (*Centralbl. f. Gynäk.*), in which he removed a nodule, and found, after great difficulty, the bacillus in giant cells. John Homans, of Boston, gives two cases similar to mine in the *Lancet*, Feb. 5th, 1888. These are the only reported cases I can find in which the disease was previously recognised. Kümmell, however, before the German Congress, in 1887, collected 40 cases. In each the cause of operation was erroneous diagnosis. He gives as the earliest case one in which Sir Spencer Wells operated in 1862. He opened the abdomen to remove an ovarian tumour

and found tubercular peritonitis; he simply drained it, and the patient was alive in 1887. Schwartze collected 17 such cases. The age of the youngest was four. Knaggs, Esmarch, Mikulicz, and Wagner have each published cases, and all these great authorities speak equally favourably of the treatment—the best results being obtained from flushing out and immediate closure of the wound.

Mr. Tait now says that we must drop the word "tubercular" concerning peritonitis, as this disease has no kind of association with tubercles in the lung. However, as to the pathology of it we are uncertain, if not entirely ignorant.

I look upon it as a distinct form of tubercular degeneration, the same as met with in joints and bone, and if dealt with early, as in these cases, by operation you will have as simple a cure; but, as in joints, so it is in the peritoneum—if we procrastinate with expectant and palliative treatment, the result will be most unsatisfactory, and such treatment must be looked upon in future as a surgical crime. At this stage in a joint you scrape and if necessary excise it, and although we can now remove many offending organs from the abdomen with impunity, we cannot remove the peritoneum. As to diagnosis, statistics show us that many eminent authorities have made repeated mistakes, and the conclusion I have drawn from that is—when I meet a case of distended ascitic abdomen I would not under any circumstances tap it. It is my practice to open it, "prepared for any emergency," by an incision sufficient to admit my forefinger, which will enable an experienced surgeon to ascertain in a minute the exact condition of affairs inside. Should it only happen to be simple ascites the result will be sometimes cure; and in any case the fluid will not reaccumulate for a much longer period than if tapped. In malignant disease it gives a complete abeyance for an indefinite period of the agony and urgent symptoms, and in some cases complete disappearance of the growth. This may seem to some of you heroic and incredible, but I can assure you, from personal experience, that it is fact; and that by this treatment not only do you know the exact state of things, but you are running less risk, if any, than

plunging a trocar into a distended abdomen, and you are certainly more merciful to your patient. As to the physiology of this method of cure we are equally in the dark, and we have so far only to accept the fact.

In conclusion, I argue from my own experience and that of others far more competent to judge, that when tubercular or chronic suppurative peritonitis is patent, and that a fair trial has been given to medicinal treatment, it is our duty as honest surgeons to open and flush out the abdomen. The operation in itself being a simple one, but strict attention to detail being more essential, it cannot safely be attempted in general practice. Of the fact that expectant and hopeless treatment is daily persisted in I am painfully cognisant, and I may have more opportunity than many of you in seeing how hard it is to dispel the absurd idea from men's heads that the peritoneum is not still the sacred structure that we were taught it was, and, though fully aware of their inability to relieve under those conditions, on conscientious grounds are afraid to recommend their patients to undergo operation. Is there one amongst us here to-night who has not sometime in his experience met with such cases, and felt how powerless we were to deal with them, while they sank daily before our eyes? I must say I look back with regret to my ignorance of this treatment, but most hopefully to the future. This Section can do a great deal to further this, and impress, through its members and literature, on the busy, hard-worked practitioner, that this and other abdominal disease is no longer incurable, and that in cases recognised early and sent up for treatment operation is not only justifiable but certain of success. Then, and not till then, will suffering humanity have a more extensive benefit of the great strides our art has made in its abdominal branch.

FRACTURED PATELLA, TREATMENT BY EXTRA-ARTICULAR SUTURE.

By T. MYLES, F.R.C.S.;
Demonstrator of Anatomy, Trinity College, Dublin;
Surgeon, Jervis-street Hospital.

[Read in the Section of Surgery, February 22, 1889.]

I FEEL tempted to apologise to the Section this evening for bringing before them such a threadbare subject as fractured patella. So much has been already written about it by better men than I am, that I fear I must trespass upon the kindness and good nature of the Section in asking them to listen to the few observations I mean to make to-night upon the subject.

I propose to discuss only four points in connection with this injury to-night, but I think you will agree with me in considering them of primary importance.

1. What is the prospect to the average day-labourer when he fractures his patella, and the so-called ligamentous union results?

2. Why is bony union so rare in this accident?

3. How can bony union be obtained?

4. Under what circumstances is the surgeon justified in adopting severe measures to obtain this result?

In spite of all that has been said to the contrary, I venture to assert, as the result of my observations of cases long after they have left the hospital, that to the man who has to earn his bread by laborious toil a fractured patella means permanent disablement. In almost every case that I have seen such has been the result.

I do not mean to assert that there are not cases in which men have been able to follow laborious occupations after the injury, but I do state that such cases constitute but a small minority, and cannot justifiably be made the basis of a prognosis in any given case.

As there is a possibility of this statement being misunderstood, I will define the position I take up a little more clearly.

When one is shown a case in hospital of a so-called successfully treated fractured patella, the patient is told, by way of illustration, to walk up and down the room, to swing his leg about, to stamp upon the ground, to flex his leg upon the thigh, and to perform other evolutions of an equally innocent and harmless character, but almost absolutely valueless as a test of successful treatment.

There are three tests which ought to be applied to such a patient before a success in treatment can be claimed by the surgeon:—

1. He should be asked to spring off the floor by an effort of the injured limb alone.

2. He should be asked when lying on his back or sitting on the bed to raise the heel from the ground, when the leg has been previously fully extended upon the thigh.

3. The band between the fragments should be felt to become tense and hard when the patient, in walking, bears upon the injured limb.

I have gone to the trouble of testing these three points on a number of cases, and the results of my observations are as follows:—

I only once met a patient who could spring a foot off the ground, and he had met with the accident in early life—in fact, at eleven years of age—and, although considerable separation between the fragments existed, he could do nearly as much with the injured as with the healthy limb.

In his case, probably, the reparative power of early youth was responsible for the successful issue.

In every other case I have examined, the patient was unable to make any attempt at jumping from the ground, and in most cases, with what I think commendable prudence, refused even to make a vigorous or determined effort to do so.

The second test, raising the heel from the ground when the leg is fully extended, is very rarely accomplished. I confess I was very much astonished to find how few patients could do it.

They will raise the heel readily enough when the foot is flexed, but not at all when fully extended—in other words, the value of

the quadriceps as an extending force is practically *nil* when the patella is broken and the fragments appreciably separated.

The third test is only an amplification of the second. The patient may be able to walk fairly well, but if you put your hand upon the uniting bond between the fragments you will notice that it is in many cases quite lax when it ought to be tense in extension of the leg. In fact, the power of the quadriceps is transmitted, not through the bond of union, but through the lateral expansions which pass down from the sides of the patella to the top and sides of the tibia, and the advancement of the injured limb in front of its fellow is accomplished probably by the psoas and adductor muscles.

A patient may be able to walk fairly well—in fact, deceptively well—but if he fails in the three tests I have given it is obvious that he will be unable to earn his bread by laborious toil, and my assertion that he is permanently disabled will be fully justified.

I have gone to considerable length to attack the so-called ligamentous union, and my reason for doing so is simply this:—One of the commonest statements in the text-books of surgery, and one of the most frequently reiterated assertions at cliniques, is, that ligamentous union is all that can be hoped for in fractured patella, and that such a result is to be considered satisfactory. I have read and heard this statement again and again, and, with the simple credulity of youth, believed it implicitly.

From this state of assured bliss I was once rudely awakened by a poor woman coming to me for a note for the Union. Her husband had broken his patella some time previously, and was now unable to earn his bread. He had been treated in the orthodox manner, with a back splint, pads, and bandages, and had left the hospital apparently well, with the fragments closely approximated, but six months afterwards a hideous gape had developed between the pieces of bone, his leg was wasted, and he was barely able to walk with the aid of sticks.

This may have been an exceptionally bad case, perhaps, but it at any rate had the effect of focussing my attention upon such cases, and as the result of my subsequent experience I do not

hesitate to say that the so-called ligamentous union is only too frequently, so far as the dynamics of the leg are concerned, a mockery, a delusion, and a snare.

I now come to the most interesting of the four questions I have set down for discussion—namely, Why is bony union so rare?

From amongst a host of explanations I select four as worthy of consideration:—

1. Non-apposition of the fragments from inability to overcome the contraction of the quadriceps muscle.

2. Non-apposition of the fragments, due to prolapse between them, under the influence of atmospheric pressure of the tissues in front of the patella, as described by MacEwen.

3. Separation of the fragments by the synovial or bloody effusion and defective formation of new bone, due to the fact that the fragments are constantly bathed in the synovial fluid of the joint.

4. Defective reproductive power in the bone itself, due either to the defective supply of blood or the absence of periosteum on the post surface of the patella.

These four will, I think, include the more important theories usually advanced to explain the non-union.

The two last are knocked out of court at once by one fact—namely, that in comminuted fractures, where of course these conditions as regards blood supply, periosteum, and synovia are found, bony union occurs fairly frequently.

The first—that muscular spasm is responsible for the non-union—will hardly hold water.

Every practical surgeon knows well that but a very slight effort is necessary in an ordinary case to approximate the fragments when the muscle is relaxed.

Any one of the many ingenious devices used in practice will overcome the contraction of the muscle and keep the fragments apparently together. I am sure that many surgeons present will support me in this statement.

MacEwen's theory, that the torn structures in front of the bone will, under the influence of atmospheric pressure, be forced into the cleft between the fragments, is no doubt a very plausible one;

I should be inclined to say more plausible than probable. Of course nothing but actual dissection of recent cases can determine the question, but from *a priori* reasoning the theory seems to me to be untenable.

A glance at a frozen section through the knee-joint will show that the tissue in front of the bone is very thin, and it is almost impossible to conceive that the torn fibres of such tissues could exert the extraordinary influence he attributes to them.

I have endeavoured to test this point by experiments upon the cadaver, but in vain; in every case I find that the muscles of the thigh will tear before the patella will break. We can only, therefore, in the absence of actual dissections of recent cases, accept Mr. MacEwen's theory as a hypothesis.

I think I have now fairly shown that none of these theories, with the exception of the last, perhaps, can be considered a sufficient explanation of the absence of bony union.

To what, then, is it due?

I would suggest, subject, of course, to correction, that in many cases the treatment is largely responsible for the bad result.

When discussing the question of muscular spasm, I stated that almost any one of the ingenious mechanical appliances devised for the treatment of the injury will overcome the contraction of the muscles and approximate the fragments, and so they will; but they will also do something more, as I will now make clear. If pressure be made upon the upper end of the patella, so as to force it downwards, it will be noticed not only to travel downwards, but also that the lower end tilts forwards. Anyone can demonstrate this in his own leg if he takes care to relax the quadriceps.

Further, the lower fragment sinks towards the tibia by its own weight, and pressure of any apparatus now forces it towards the cavity of the joint. The result, therefore, is that the fragments are now lying in totally different planes. Is it any wonder, therefore, that bony union does not result? The real matter of astonishment, I think, is that any kind of union at all results.

Now, as far as I can see, no splint or external mechanical appliance can be devised that will remedy this, because it depends

upon constant anatomical conditions. The useful frozen section shows us that the posterior border of the patella is convex from above downwards, and the patellar surface of the condyles is also convex from above downwards. That being so, any attempt to shove down the upper fragment by pressure from above and in front will cause the rotatory movement I have endeavoured to describe.

How, then, can we get the broken surfaces together? The ready answer is—by opening the joint, drilling the bones, and wiring the fragments together after the manner practised by Sir J. Lister.

To this there are, however, very decided objections. At present there appears to be a superstition current that provided you have a spray going, gauze dressings, drainage-tubes, perchloride solutions, and other necessary paraphernalia, you can cut into a knee-joint and do pretty well what you like with it.

Professor Dennis, of New York, has collected the records of 186 cases in which the joint has been opened and the fragments wired, and the results are interesting indeed. Of these 186, 11 died, 4 resulted in amputation, 34 suppurated, 31 had either partial or complete ankylosis; in 24 the result is described as poor, in 35 as fair, and in 75, or less than half, as good.

Now these results are not encouraging, and I think most of us as yet would hesitate to recommend Sir Joseph Lister's method as a primary treatment.

In a discussion some years ago on the same subject, Dr. Robert M'Donnell put the question in a nutshell when he asked which of us, if his patella were fractured, would permit the joint to be opened and the fragments to be wired together? In a crowded meeting there was only one answer in the affirmative, and that was from my friend Dr. J. Lentaigne, who, with a martyr-like enthusiasm, declared his willingness to submit to the sacrifice provided always Sir Joseph Lister was the officiating high priest.

When cogitating over these things it occurred to me that it was possible to bring the fragments together and keep them there without opening the joint; that it would be possible to obtain the advantages of Sir J. Lister's method without the dangers.

Through the kindness of Professor Cunningham I was given ample opportunity for testing the feasibility of my plan on the cadaver before trying it on the living.

My method, which I have ventured to designate "Extra-Articular Suture," is simply this:—

The limb being cleaned, the skin is drawn well up; a small puncture is then made through the skin on either side with a common bradawl; the upper fragment is cautiously drilled from side to side, the drill being constantly kept accurately parallel to the anterior surface of the fragment; through the drill-hole a stout nickel silver pin is passed, its two ends project on either side of the fragment, its centre is buried in the substance of the fragment. The lower fragment is then similarly drilled and transfixed, the skin having been previously drawn well down.

With the pins now in position you have a powerful grip on the fragments, and it is astonishing how little force is necessary to overcome the muscular contraction. When the fragments are drawn together and fixed in proper apposition, a gauze tape can be passed under and around them in a figure of 8, as in a hare-lip operation, or the free ends can be securely wired together.

A thick pad of lint, dipped in perchloride solution, 1 in 500, is wrapped around the joint, and outside that a layer of gutta percha tissue, to delay evaporation.

A posterior splint and flannel roller completes the dressing, which is not stirred for five or six weeks.

After the preliminary puncture and drilling the treatment is absolutely painless, as my patients have again and again asserted.

Such is briefly the method I employed in the first of my series of cases, and it is the method I hope to follow in subsequent ones.

In two other cases I modified the method slightly. Instead of drilling the bones I passed the pins between the bone behind and the expansion of the vasti muscles in front, hoping that by doing so I would obtain as good a grip on the fragments as in the first method.

The result, however, was disappointing in these cases, the tissue in front of the patella not giving evidently a sufficiently good and enduring grip to the pins.

I have abandoned that method now, and will adhere to my original idea in future cases.

The advantages I claim for the method are :—

1. That it is effective—that is to say, it will keep the fragments in perfect apposition without the tilting incidental to mechanical appliances.

2. It is free from danger of septic infection, as the joint is not opened.

3. It is easy and simple and within the reach of everyone, requiring no costly appliances.

The only methods that I know of that it can fairly be compared with are Professor Volkmann's method and Mr. Treves' modification of Malgaigne's hooks.

Professor Volkmann's method consists in passing a wire ligature through the border of the quadriceps above and the ligamentum patella below.

The objections to this method are so obvious that I will not detain you by detailing them.

Mr. Treves' method of using Malgaigne's hooks is open to several objections :—

1. Everybody has not and may not be able to procure the hooks.

2. The joint is often perforated above or below.

3. The hooks also tend to tilt forwards the lower end of the upper fragments as they press the patella from the front and above.

4. They are painful and cumbersome.

I think I may, without either arrogance or conceit, assert that with either of these methods the plan I suggest will bear comparison of safety, efficiency, and simplicity, and to be regarded as the basis upon which a decision is to be given.

I will now conclude by stating that I honestly believe if bony union can be obtained by any means short of opening into the joint it will be obtained by the Extra-Articular Suture.

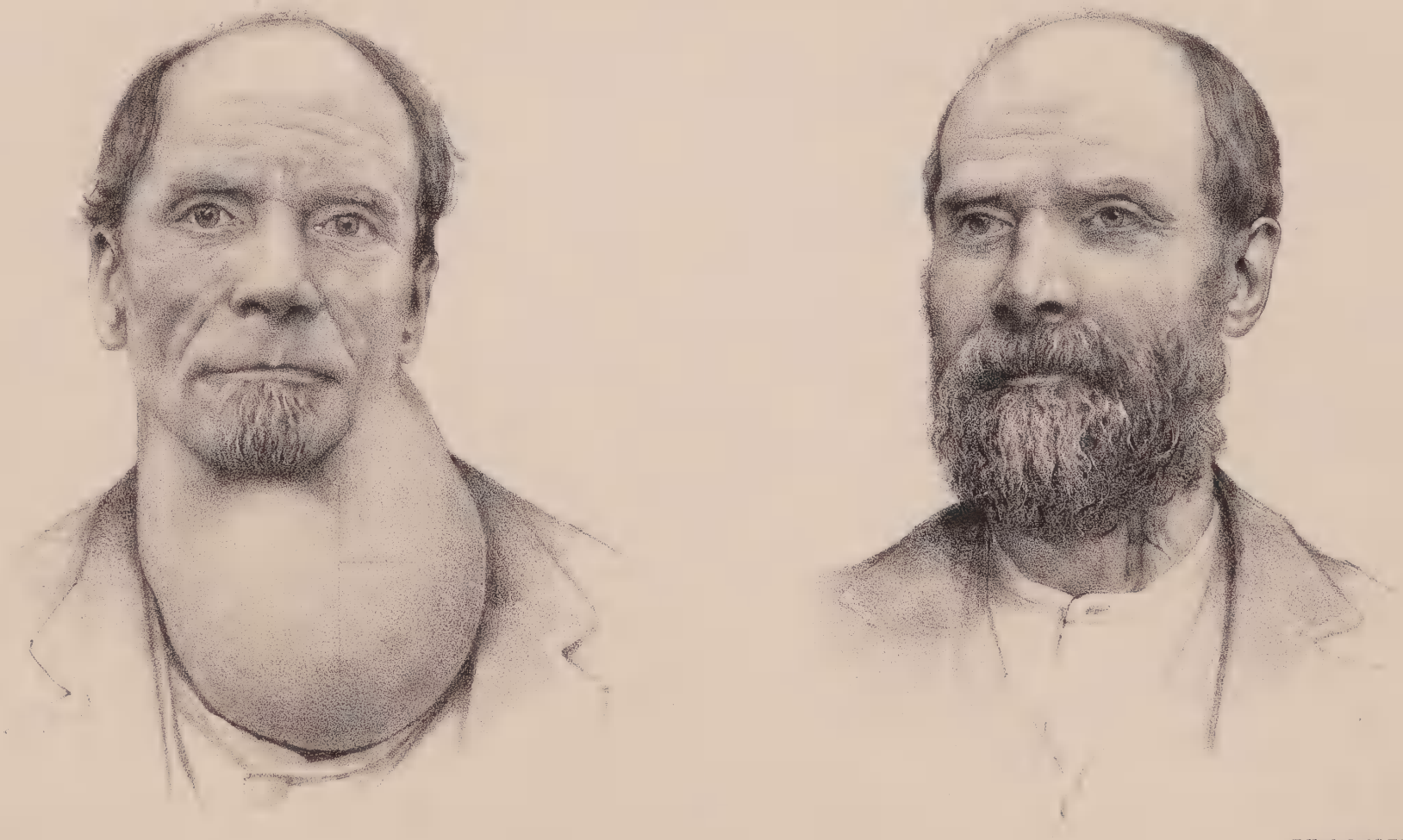
UNUSUAL CASE OF THYROID TUMOUR.

By ANTHONY H. CORLEY, M.D.
Surgeon to the Richmond Hospital.

[Read in the Section of Surgery, March 29, 1889.]

The case which I submit to-night to the notice of the Surgical Section is one which, with respect to history, diagnosis, treatment, and result, presents features worthy of discussion.

On the 16th of February last Dr. Byrne, of Derry, wrote to obtain my opinion on a case of tumour of the neck, which he had been attending, in consultation with Dr. M'Laughlin, of that city. The patient was sixty-two years of age; and the lump had been growing for nearly thirty years. For the last two years its rate of increase had been unusually rapid, and it had begun to interfere with his respiration and his voice. Dr. Byrne had, in view of these threatened dangers, recommended puncture and aspiration, for exploration as well as for relief. This procedure was adopted early in February, 1888, and about eight ounces of fluid were drawn off. The size of the tumour was reduced, but in a few days it was as large as before. The fluid evacuated was sherry-coloured, and separated, on standing, into a floating jelly-like clot and a subjacent serum. The circumference of the neck before aspiration was 24 inches, and, after the operation, 22½ inches. No microscopic examination was made. Before giving any decided opinion as to the nature or treatment of the case, I asked Dr. Byrne to send me some photographs, and these I received in a few days, together with answers to several queries which I forwarded. The photographs were most instructive, especially when taken in connection with the able descriptive letter which accompanied them. The marks on the likenesses are thus explained—the × at the



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Mr Anthony H. Corley's case of Enlarged Thyroid Gland, from photo's taken immediately before and six months after operation.

right angle of the jaw indicates the position of the larynx; the carotids could be felt beating well up on the mastoid process of the temporal bone. From the patient's own description, Dr. Byrne was of opinion that the tumour had commenced in the sub-maxillary triangle of the neck.

In giving much consideration to the case, assisted by the history and photographs, I formed the opinion, subject to subsequent modification, that the tumour was cystic, non-malignant, dangerous by mechanical pressure alone, as evidenced by the pushing aside of the principal anatomical structures of the neck, and, probably, amenable to treatment by operation.

I saw him first on the 3rd of March, and my opinion as to the necessity for, and feasibility of, operation was confirmed—a view in which Dr. Thompson, of Omagh, who saw the patient with me, fully coincided.

Accordingly, he was admitted into the Richmond Hospital on the 14th of May. He is described in the case-book by Dr. Nash, our able House Surgeon, to whom I am indebted for these notes, as "a small, but healthy, energetic man, of temperate habits and regular life, but with a permanently intermittent pulse. He believed that the tumour has been rapidly increasing for some time, but especially so for the last five months. Tumour somewhat pyriform in shape; broad end below, extending to episternal notch; skin freely movable over it, allowing very large and tortuous veins to be seen on surface; on palpation there was a feeling of deep fluctuation."

From information which I received from the patient, I was of opinion that he had unintentionally misled Drs. Byrne and M'Laughlin as to the site from which the tumour had originally sprung. This, as may be remembered, was the region of the sub-maxillary triangle. On taxing his memory, he recollected the origin was lower down, at the left side of the trachea, and that, for some time after its appearance, he was able to conceal it by wearing a high shirt-collar.

I was nearly satisfied in my own mind that I had a bronchocele to deal with, possibly cystic, but so uniform in feeling and contour that

no distinct cavities could be diagnosed. There was nothing very reassuring in this conclusion.

I have had a tolerably large experience of thyroid operations, chiefly as assistant, and the prospect of removing the gland from a man of sixty-two, with an intermittent pulse, was not an inviting one. However, I had two considerations to help me—first, that the tumour was, undoubtedly, strangling the patient; and next, that he was a man of the most genuine "pluck" I had ever met. Every possible danger connected with the operation, including that of death, was explained to him, but his invariable comment was—"I know it's going to kill me; I came up to have it done, and I'll take all the risks, if you do it." He is so well known and liked in his native town, that Drs. Byrne and M'Laughlin, his own medical attendants, came up to be with him in his hour of trial.

In their presence, on the 24th of May, ably assisted by my colleagues, I performed the operation.

A vertical incision through the skin, a little to the left of the mesial line, and one transversely from the middle of that, as indicated by the lines on the photograph, sufficiently exposed the tumour. Layer after layer of covering was divided, till the capsule came into view; and rapid enucleation with the finger and director was proceeded with. Some large veins required ligature, also one large artery towards the left maxillary portion of the tumour. By degrees the whole mass was separated from all connected structures, and the last vascular attachments were ligatured and severed. The larynx and trachea returned almost to their normal position. The œsophagus had not been pushed aside, but occupied its usual site. The left superior laryngeal nerve was completely exposed, and part of its external branch divided. Up to this the bleeding had been of no account; but a most dangerous-looking chasm, overhung by wrinkled flaps of skin, with bare trachea, œsophagus, sheaths of vessels and vertebral column at its bottom, indicated the bed of the tumour. So far, everything went well, and the operation had not lasted more than half an hour. The incisions were carefully sutured, and the patient allowed to get from under the influence of the

anæsthetic. Just as I was about to give directions to have him removed to his bed, a sudden, most alarming, gush of hæmorrhage took place; the blood welled up in dark streams between all the points of suture; the patient's face blanching, and his pulse becoming feeble.

Immediately the sutures were cut, the wound opened, and sponges pressed in to give time for the application of ligatures. This, however, was no easy matter. Although the chief gush issued from the upper end of the wound, there seemed a general flow from its whole length. A bleeding-point above was seized and ligatured, but the welling still continued, and only diminished when the patient fainted.

On consultation, we determined that plugging and pressure were the only feasible means of dealing with the hæmorrhage.

Lining the wound with a layer of alembroth gauze, I packed that with iodoform cotton, fixed with a large pad and lightly-applied bandage. In three hours there was some recurrence of the bleeding, but digital pressure restrained it. This hæmorrhage, and its necessary treatment, complicated the operation very much. It precluded anything like immediate union of the wound, and, of necessity, produced suppuration, with all its attendant dangers. For three or four days he remained tolerably quiet, drinking milk and beef tea with difficulty, and having his strength supported by nutrient enemata and zyminised suppositories.

On the ninth day after the operation restless delirium set in, which continued for twelve days. There was a strong disposition to leave his bed; sometimes, to get out of the window; and, at all times, a wish to return to Derry. The plugging was not taken out until the sides of the cavity began to granulate—even then the pulsation of the carotid could be seen at its bottom; and, with this condition, his restless movements and turnings of the head made his appearance almost appalling.

On the twenty-eighth day after operation two abscesses appeared—one on the radial side of the forearm, and the other on the gluteal region. These were opened, and, after the evacuation of healthy pus, they healed up.

On the twentieth day I endeavoured to promote secondary union, and preserve the wound from injury, during the delirium, by passing deeply a couple of hair-lip pins, and bringing the parts together by a figure-of-eight suture.

For a week before his departure from hospital he had returned to his right mind, and was slowly gaining strength; but he displayed such a constant anxiety to go back to his home, that, before the wound was healed, I allowed him to leave hospital, which he did on the 29th of June, thirty-six days after the operation. I had less hesitation in this—first, because I knew it would tranquilise his mind; and next, because his own surgeons could meet any possible complications during convalescence.

The healing of the wound was slow; and, considering that suppuration had been fairly copious all the time, it is not surprising that the deep ligatures came out one by one at intervals—and this still further delayed the healing.

Immediately after the operation we perceived a certain amount of aphonia; this remained unaltered, and is still present. The struggle which he passed through before he left the hospital is shown by this photograph, taken on the thirtieth day.

He is now in perfect health in mind and body, as is conclusively shown by his presence here to-night, but I cannot say that his case will either tend to throw additional light upon, or involve in fresh mystery, the functions of the thyroid gland.

As to the nature of the tumour I have macroscopic and microscopic evidence; it weighed 4 lbs. 4 oz. shortly after removal, but anything more dissimilar to a thyroid gland cannot be imagined, or more unlike the shape, when *in situ*. Light in colour, firm to touch, and shaped like a small apple-dumpling, it could scarcely be recognised as the pear-shaped tumour. My colleague, Dr. Scott, made the microscopic sections, which are before you. It was not a cystic goitre, as there were no large cavities in any part of it—it was a simple enlargement of the thyroid gland; and Dr. Scott remarked that if he wished to exhibit sections of the normal gland to his class, he could not have selected better than the specimens before you.

One word more. I cannot sufficiently thank Drs. Byrne and M'Laughlin for their care of this patient, and for the interest they took in the consideration of operative interference, and in assisting to bring the case to a satisfactory conclusion.

I was in hopes that this case would be a contribution to the vexed and important question of cachexia struma-priva. This curious disease (first recognised by Gull as the cretinism of adults; named by Ord, myxœdema; proved by Reverdin and Kocher to have a direct connection with the removal of the thyroid gland—proofs confirmed experimentally by Horsley) was before my mind from the date of the operation until very recently. The character of the tumour, its position, its completeness, size, and want of connections, made me believe that I had removed the entire gland. The cerebral symptoms following confirmed this view; and one of my colleagues attributed them altogether to altered functions depending on that removal.

I awaited anxiously, and received report after report as to his condition, and was equally pleased and interested at hearing there was complete restoration to health, without a trace of myxœdema. Shortly after he returned to Derry a small soft lump appeared at the upper part of the trachea, apparently enveloping it. I had the opportunity of examining him last night, and again to-day, with my colleagues. I am of opinion that the lump is formed by the isthmus; that the right lobe still remains; and that the tumour removed was the hypertrophied left lobe, which had, in its growth, produced an atrophy of the other structures of the gland. My explanation of this is as follows:—As the left lobe grew, and pushed the larynx up to its position, the left side of the isthmus was gradually drawn out into a band of fibro-areolar tissue, which, without attracting any notice, was divided in the operation. With the removal of the pressure, and the convalescence of the patient, the isthmus and the right lobe have resumed their normal size, and their presence explains the absence of consecutive constitutional disease.

ARTHRECTOMY, OR ERASION OF THE KNEE-JOINT.

By FRANCIS T. HEUSTON, M.D., F.R.C.S.;
Surgeon to the Adelaide Hospital and Lecturer on Anatomy, Carmichael College of Medicine.

[Read in the Section of Surgery, March 29, 1889.]

Case I.—R. L., aged eight years, was admitted into the Adelaide Hospital on Monday, 7th May, 1888, with separation of the superior epiphysis of the tibia, which had occurred on the preceding Tuesday in the following manner:—The patient, while endeavouring to escape from two other boys in a school-room, tried to force his way between two desks which were then pushed together, his leg being caught between the two; in attempting to wrench his leg free the patient felt something give way, but did not suffer from much pain at the time. He was kept at home and treated by hot stupes, owing to his knee-joint having become greatly swollen. On his admission to hospital his injured knee was two and a quarter inches greater in circumferential measurement than the sound one, and presented the evidences of acute synovitis. At this time free movement was to be obtained between the superior epiphysis and shaft of the tibia. He remained in hospital under treatment until June 21st, when the limb being enclosed by a plaster-of-Paris splint he was allowed home. This was removed on July 31st, he having progressed favourably, and the separated epiphysis united. Subsequently he was able to walk without pain, and continued in good health until November, when he was again admitted to the hospital, owing to sudden pain and effusion into the knee-joint, which had occurred without any apparent cause. He remained under treatment for this condition until December 18th, without any relief; his temperature persisting between 100° F. and 102° F.; a hypodermic needle was now passed into the joint, which was found to contain pus. On December 28th I made an incision into the joint to the inner side of the patella, intending to drain, but found after the pus had been evacuated that the exposed portion of the condyle of the femur was covered by a

vascular membrane, and that vascular fringes protruded through the opening; the incision was now extended across the ligamentum patella as in the ordinary crescentic incision for excision of the joint. When the patella was reflected the articular surfaces of the femur were found to be covered by a thick vascular membrane, while the synovial membrane elsewhere was also greatly thickened and vascular. All the implicated synovial membrane and cartilages were carefully removed by the scissors and scoop, the joint washed out with corrosive sublimate solution, and drainage tubes inserted from the popliteal space, after which the ligamentum patellæ and capsular ligament were united by hidden sutures of gut. On Friday, 28th, the wound was dressed, when it was found to have united throughout, the drainage-tubes being then removed. Subsequent to operation the temperature was normal, except on the morning of the 21st, when 102·2° F. was registered; it was, however, normal that evening.

On January 19th (thirty days after the operation) the limb was encased in plaster-of-Paris, there being then no pain in the joint, and the patient was discharged from the hospital on the 24th.

Six weeks subsequent to this he applied at the hospital to have the plaster removed, when the joint was found to have slight movement.

Case II.—E. S., aged six years, was admitted to the Adelaide Hospital on January 7th, 1889, with the following history:—Two years ago the patient fell on her right knee, which caused an inflammation, for which she was treated by the army surgeon attached to the regiment her father was serving in while stationed in Preston. His treatment extended over a period of eighteen months, but the swelling and pain still remained in the joint. About two months before admission to hospital the regiment was ordered to Dublin, where the patient was seen by another surgeon, who advised that she should be taken to the Adelaide Hospital for treatment.

On admission the knee presented the characteristic local evidences of tubercular disease implicating chiefly the synovial membrane.

The patient was kept under observation until January 19, when, the joint being opened, the synovial membrane was found greatly thickened and studded over by villous-like growths, while the cartilages presented localised evidence of erosion. The diseased tissues were freely removed, as in the former case, care being taken to remove the portions of eroded cartilage, but the semilunar cartilages

were not removed, they being apparently healthy. Drainage-tubes were inserted from the popliteal space, the various ligaments sutured, the joint asepticised, and dressings applied.

The dressings were changed and drainage-tubes removed on the 23rd.

The subsequent progress of the case was favourable, the temperature never rising above 99° F., and on Feburary 15th (twenty-seven days after operation), the wound being perfectly healed, the limb was placed in a plaster-of-Paris splint.

On March 2nd the plaster was removed, and, the limb feeling quite strong, the patient was allowed about with only a bandage applied, and was discharged from hospital on March 17th, having a movable joint and no evidence of disease.

The operation of arthrectomy is one which we may reasonably expect will occupy a prominent place in conservative surgery, as it holds out, in certain cases, a means of arresting morbid action in joints before the disease has advanced to such a stage as to render it questionable if the patient's life can be saved by any means short of amputation, and although much has been obtained in this direction by excision, the results of which have during the past few years been so favourable, still we have in arthrectomy an operative procedure, especially in early life, and at an early stage of disease, which is not so formidable, and has the advantage of giving in all probability a movable joint, and certainly no subsequent arrest of growth in the limb. The cases requiring this operation are those in which the usual constitutional and local means of treatment have been found incapable of arresting the gradual extension of disease in the joint, and the question of operative interference is forced on the mind of the medical attendant, then a timely arthrectomy will arrest the disease and save the joint from the more formidable operation. I have now performed the operation on three occasions. The history of the first two I have here given, and have exhibited the patients. The third, an adult, is at present under treatment in the Adelaide Hospital, and although the disease had advanced to such an extent as to require the removal of the semilunar cartilages, and scooping the internal articular surface of the tibia, the patient is progressing favourably.

In 1881 Wright commenced to perform this operation, and in 1885 published the result in 16 cases, which, stated shortly, is:— One recovered with a movable and painless joint, six with sound and useful limbs, two were partially successful, five required subsequent amputation or excision, and two died from intercurrent diseases. His more recent cases, however, give a much better result.

Müller, in 1885, recorded the result in twenty-three cases from König's clinic, operated on between 1881 and that date. One died from iodoform poisoning, one from chloroform, and two from subsequent tuberculosis. Of the remaining nineteen cases, fifteen were cured locally (in five of these subsequent minor operations, as curetting, &c., were necessary); of the remaining four two had slightly secreting sinuses and two were subsequently reported as being well. Of eight cases of synovial tuberculosis only one failed to give primary cure. In ten cases with co-existing osseous foci four cured rapidly and six slowly. In eight only of the nineteen cases (all of which had osseous foci) did firm anchylosis result, while in two considerable active flexion and extension remained.

Gerster operated on three cases, in two of which, one an adult, the tubercular disease recurred, while the third was permanently cured.

Shield had also three cases; one recovered with good and useful limb, one died from acute tuberculosis, while the third required subsequent amputation, owing to chronic flexion of the joint. Mandry states the result of seven cases, in four of which primary cure resulted, in two circumscribed suppuration followed, and in one excision and scraping of fistulous tracts had to be subsequently resorted to; in one of these cases a tuberculosis focus had been overlooked, and excision became necessary at a later period. The final results after a period of from $2\frac{1}{2}$ to $3\frac{1}{2}$ years showed in one case almost normal mobility of the joint, and in the remaining five anchylosis. In three of the cases there was no difference in the length of the limbs, while in one there was slight shortening and in two slight elongation. This operator collected sixty-three further cases, chiefly from those of Volkmann and König, and gives the fol-

lowing analysis of the whole :—10 per cent. died of diseases not connected with the operation ; 27 per cent. were unsuccessful owing to recurrence of the disease ; 63 per cent. were cured. Of the 44 cases which did well, eight recovered with movable joints, and six of these with almost normal mobility.

LINEAR PROCTOTOMY.

By CHARLES B. BALL, M.Ch., F.R.C.S ;
University Examiner in Surgery ; Surgeon to Sir Patrick Dun's Hospital.

[Read in the Section of Surgery, June 7, 1889.]

Linear proctotomy or external rectisection is an operation which has not, I feel convinced, met with the support from the profession in the United Kingdom which it merits ; and except as a preliminary to amputation of the rectum is but seldom had recourse to.

It may be defined as the making a deep incision through the anus and rectum directly backwards to the coccyx and hollow of the sacrum, and the following are probably the chief indications for its performance :—

I. For the cure of non-malignant strictures of the rectum in which gradual dilatation by means of bougies, or other instruments, is either impracticable or inefficient.

II. For the removal of new growths situated high up in the bowel or in the hollow of the sacrum.

III. As a palliative measure, instead of colotomy, to relieve obstruction in advanced malignant disease.

IV. As a preliminary step to amputation of the rectum.

The treatment of rectal stricture by bougies, mechanical dilators, or expansile bags, is, in many instances, eminently unsatisfactory ; and we occasionally meet with cases in which it is impossible to dilate the constriction to any appreciable extent, consistent with the avoidance of rupture, while in other instances the bougies give rise to so much irritation that their use has to be discontinued ; so that in order to overcome the difficulty something more is required, and the operations which have been practised with this view are—internal section, linear proctotomy, excision, and colotomy. Internal section, by means of a guarded, blunt pointed knife, has met with but little support, and it is open to serious objections.

In the first place—if the incision, or incisions, are confined to the thick ring of indurated tissue constituting the stricture, but little relief is afforded, owing to the tough and resisting nature of the structures; while, if they extend into the healthy tissue outside, fæcal extravasation and stercoral abscess will be the necessary result. In this respect the treatment of rectal strictures offers a marked contrast to a similar condition in the urethra; in the latter the treatment by internal incision is probably the most favoured operation, and we know that it but rarely is followed by urinary extravasation or urethral abscess; the cause of this difference is not far to seek. When the urine is enabled to pass the strictured urethra by means of the incision it escapes freely, as it has no further obstruction to overcome; and in passing out at the meatus it merely follows the course of least resistance; but in the rectum the fæcal mass, after passing the stricture, has to distend the anus before it can escape, and a considerable amount of pressure is necessary for this purpose. In the effort to accomplish this some fæces will be extravasated, to a certainty, if the internal incision has penetrated through the rectal wall. When, however, linear proctotomy has been performed, and the incision carried, not only through the entire stricture, but also through the lower portion of the rectum and anus, no resistance is offered to the fæcal mass, and extravasation is an impossibility. Excision of non-malignant strictures can scarcely be defended, as any cases situated low enough down to be effectually dealt with in this way can be admirably treated by linear proctotomy. Where, however, the stricture extends very high up, colotomy must still be looked upon as the only resource.

As it has frequently happened that fistulæ in connection with stricture have been operated upon without the stricture being diagnosed, and as the internal openings of these fistulæ are occasionally situated above the stricture, it is evident that complete division of the contraction has been thus sometimes unintentionally performed; but the first deliberate attempt to cure stricture in this way appears to have been made by Mr. Humphry, of Cambridge,[a]

[a] Association Medical Journal, p. 21, 1856.

who proposed this method in consequence of the "good results following longitudinal incisions in urethral stricture." And although he performed the operation on two occasions with excellent results, this method of treatment appears to have fallen into disuse until comparatively recently. To M. Verneuil is undoubtedly due the credit of having revived this operation, and of having brought it prominently before the profession. In a paper read at the Surgical Society of Paris,[a] he enters very fully into the subject, and enumerates ten cases of the operation.

Can we anticipate a complete and absolute cure by this treatment in cases of non-malignant stenosis? To this question I am convinced that the answer can be given in the affirmative. If the operation has been completely performed, that is, if the entire thickness of indurated structures has been divided, a permanent cure will result in the majority of instances, and, as compared with colotomy, the relative mortality is slight. After the recovery of the patient the power of controlling the evacuations is generally fairly well regained, so that the occurrence of incontinence is improbable. The operation may be performed by different methods. Verneuil recommends the following plan:—The bowel having been well cleared out, and the patient placed in the lithotomy position, the finger is introduced through the stricture. If difficulty is experienced in doing this, a probe-pointed bistoury is first passed, and an incision sufficient to allow of the easy introduction of the finger is made in a direction directly backwards. A trocar and cannula is now entered at the tip of the coccyx, and pushed on till it enters the rectum well above the seat of stricture. A flexible bougie, or piece of string, is by means of the cannula passed into the rectum, the end being hooked out at the anus with the finger. By means of this the chain of an écraseur is now passed, and the tissues thus surrounded are gradually divided. More recently Verneuil[b] has recommended the opening into the rectum to be made with a fine point of Paquelin's cautery, instead of the trocar and cannula. Van Buren is of opinion that the entire section is

[a] Bull. de la Soc. Chirurg., October, 1872.

[b] International Medical Congress, Copenhagen, 1884.

best made with a knife-shaped cautery, the charring of the edges protecting the wound from irritation of the fæces until granulation is established: he also avoids making it *directly* backwards, as he considers that the wound heals better when made a little to one side. Of all methods the knife is, I am satisfied, the best. With a probe-pointed bistoury the division can be more surely, expeditiously, and cleanly made than by any other means; and if it is confined to the middle line there need be no fear of bleeding. Any vessel which does spring can easily be ligatured; or, if there is general oozing, the wound may be plugged with an aseptic sponge, or, better still, with iodoform gauze, and the whole supported with a T-bandage. At first there will be a certain amount of incontinence, but as the wound heals the power of retaining fæces will gradually be regained. During the healing, frequent syringing with some antiseptic solution, preferably corrosive sublimate (1 in 2,000) should be employed; or, until granulation is established, continuous irrigation with warm solution may be resorted to.

The following two cases are good examples of this operation for the relief of non-malignant stricture:—

Case I.—A gentleman, aged twenty-three, consulted me in December, 1887. He had suffered from syphilis five years previously; he was pallid and emaciated, and complained of the greatest difficulty in defæcation, and had a continuous discharge of bloody mucus from the anus. Digital examination showed a ring of ulceration round the entire circumference of the gut, most marked in front, and immediately above the ulcerated surface a tight stricture was to be felt; the tip of the index finger would not pass into it; and it felt hard and rigid. Attempts to dilate with bougies gave rise to great irritation, and rather aggravated his condition. His abdomen became distended, and he had some vomiting. With the assistance of my colleague, Professor Bennett, and Dr. Browne (December 27, 1887), I divided the stricture posteriorly, the incision passing up into healthy tissue above, and extending through the anus to the tip of the coccyx and into the hollow of the sacrum. The relief to the symptoms of obstruction was immediate, and he made a good recovery. Defæcation now is

normal; he only has a very slight amount of incontinence if he gets an attack of diarrhœa; the ulceration has quite healed, and there has been no recurrence of the stricture; he is quite strong, and looks thoroughly healthy.

Case II.—Although rather at a loss to define the pathological condition in this case, the indications for linear proctotomy were obvious:—

Mrs. A., aged forty-seven years, recommended by Dr. Kenny, of Killeshandra, consulted me, February, 1886. Three years ago she noticed right breast becoming shrunken and nipple retracted, but she never had any pain in it; the left breast became similarly affected about eighteen months subsequently, and several nodules appeared over abdomen, which disappeared, leaving puckered subcutaneous cicatrices. Since then she suffered from ascites, and has been tapped six times; and for the past twelve months has had gradually increasing difficulty in getting the bowels to move. When I first saw her, her condition was as follows:—Appears well nourished, active, and healthy, has no pain, and only complains of discomfort when ascites becomes considerable. Breasts, nipples both quite retracted, the glands themselves feel somewhat nodular, but not hard; the skin is puckered and adherent, with what look like subcutaneous cicatrices—there is a similar condition in several places over the abdomen. The abdominal cavity contains a moderate quantity of ascitic fluid, but is not fully distended; the upper portion of the vagina is contracted and rigid, and the uterus immovable, but the surface smooth and free from nodules. A smooth stricture of the rectum exists $2\frac{1}{2}$ inches above the anus. By the use of aperient medicine, and occasional passage of bougies, her condition was somewhat improved, but the contraction of the rectum gradually increased; and by the end of 1887 symptoms of intestinal obstruction became developed, with vomiting and great abdominal distension. Accordingly, as the obstruction was obviously tending to her death, I performed linear proctotomy, assisted by Drs. Kenny and Bennett, in January, 1888. The relief to the symptoms of obstruction was immediate and complete, and she gradually recovered, the ascites quite disappearing. She subsequently went, for the summer, to Bundoran, and died there, with symptoms of obstruction to the œsophagus, having had no recurrence of the symptoms of intestinal obstruction. Unfortunately there was no opportunity of an autopsy in this most remarkable

case. I have never met with a description of any case like hers; it certainly was not carcinoma, but appeared more to be a condition of disseminated cirrhosis of the various structures implicated, in some respects resembling the cheloid of Addison.

As examples of the removal of neoplasms by the aid of linear proctotomy the following are instructive:—

Case III.—A man, aged sixty-three years, was admitted into Sir Patrick's Dun's Hospital, October, 1884, with piles, fissure, and history of obstruction in the rectum. Digital examination revealed the presence of a tumour occupying the hollow of the sacrum, and compressing the rectum towards the bladder. As the finger could be passed above the upper limit of the growth, an attempt was made by linear proctotomy to remove the tumour, or, at any rate, to relieve the obstruction, the symptoms being so urgent that either this operation or colotomy was indicated. The incision was made in the usual manner, and, by means of the finger and scoop, the tumour, which proved to be in part a lympho-sarcoma, was broken down, and as much as possible removed, the obstruction being completely overcome. As hæmorrhage was free the wound was plugged, but unfortunately the patient got diffuse pelvic cellulitis, followed by peritonitis, and died on the sixth day.

Case IV.—October 4, 1887, I saw, with Dr. Wright, of Dalkey, and Drs. Finny, O'Farrell, and Bennett, a gentleman, aged twenty-six, who had been suffering for some months from blood-stained diarrhœa, with abundant discharge of intestinal mucus; he was extremely emaciated, blanched, and obviously his life was in very grave danger. Digital examination revealed a soft growth at the upper portion of the rectum, but so high up that its details could not be made out or its limits fixed. I advised that the only prospect of saving his life lay in the attempt to remove the mass by the aid of linear proctotomy, which was accordingly performed the next day. Immediately after the incision was made he became pulseless, and his respiration was very shallow, so that we had to resort to artificial respiration for a considerable time. By the facility afforded by the incision the growth could be easily explored; it was about the size of a florin, soft, and extremely vascular. In his enfeebled condition it was obvious that any attempt to remove it, if attended with further hæmorrhage, would be immediately fatal, so I destroyed it as completely as possible with the actual cautery.

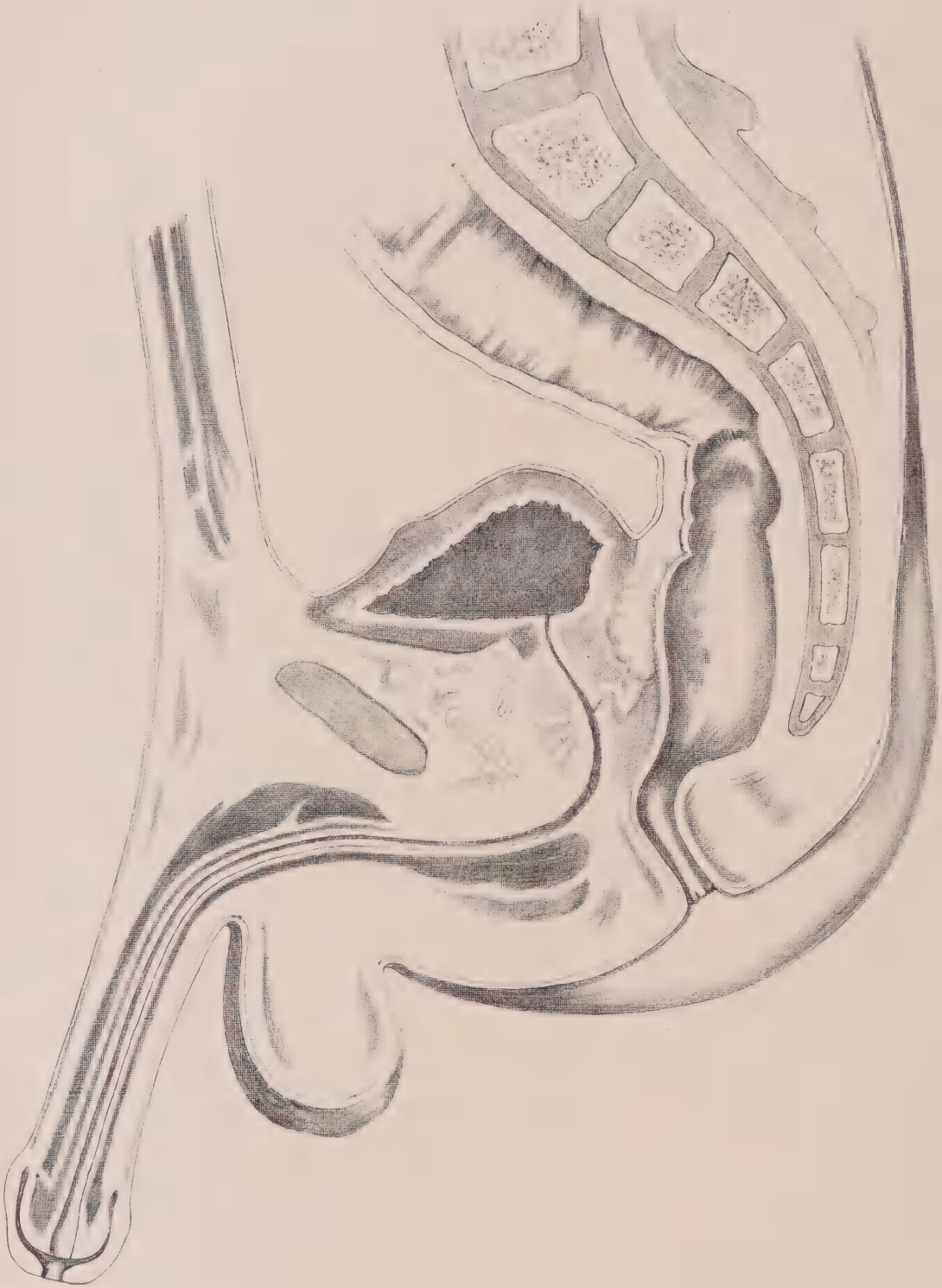

DIAGRAM OF VERTICAL SECTION OF THE MALE PELVIS.

Drawn to Scale from the Average of three Frozen Sections.

Falconer

Dublin

The patient made a slow but eventually perfect recovery, and a year subsequently was in good health, able to walk long distances; no incontinence of fæces, and no evidence whatever of recurrence.

The recommendation of Verneuil and Kelsey to treat obstruction due to malignant disease by linear proctotomy is not likely to be largely followed, and is, I believe, not good surgery. For, in the first place, if the disease is low enough down to be benefited by linear incision, it is probably better treated by excision; and even if the latter operation be contra-indicated by any other condition the advantages to be gained by diverting the fæces from a hopelessly cancerous rectum by means of a well-timed colotomy are now pretty generally admitted.

Linear proctotomy as a first step of amputation of the rectum is now, I think, practised by most surgeons, and in all cases in which I have operated for the past six years I have employed it. The advantages are—complete command over the rectum, easy performance of the further steps of the operation, and last, but not least, it provides a thoroughly efficient channel for drainage subsequently. The operation of linear proctotomy is one by no means devoid of danger; the incision necessarily penetrates into the loose areolar tissue of the pelvis, so that the risk of septic absorption is real—as was demonstrated by one of the cases above recorded. This case, however, occurred several years ago, before the efficient antiseptic measures now employed had become evolved. But, on the other hand, when successful, it may be the means of saving many a valuable life. In order to appreciate the full advantage to be gained by this procedure it is necessary to refer to anatomical details, and for this purpose it is better to make use of frozen sections, as by this means a truer estimate can be found than by any other method of dissection. Through the kindness of Professor Cunningham I am enabled to produce a diagram, which is drawn accurately to scale and is the average of three vertical sections of the male pelvis. It will be seen that there is a very considerable amount of tissue between the anus and the tip of the coccyx—the Ano-coccygeal body of Symington. This body in the female is

given by Ranney and Barbour as being relatively longer. From a glance at this diagram it will be seen that an incision through the ano-coccygeal body will open up the rectum well above the anal canal, where the gut is normally wide: and as syphilitic ulceration is usually situated in the pouch below the prostate gland, with its consequent stricture immediately above this point, it is obvious that not only is the strictured bowel completely divided, but the healthy intestine above opened up. As far as my experience goes, if the operation is thoroughly done and care taken to prevent contraction during healing, a permanent cure of rectal stricture may be looked for by this means.

ON THE TREATMENT OF ACUTE INTESTINAL OBSTRUCTION DUE TO COLLECTIONS WITHIN THE LUMEN OF THE BOWEL.

By W. THORNLEY STOKER, M.D., F.R.C.S.I.;
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[Read in the Section of Surgery, June 7, 1889.]

There is no branch of surgical treatment on which the modern practice of free operation, as fostered by antiseptics, has had a more healthy and invigorating effect than on that dealing with obstruction of the intestine.

The old methods of diagnosis have been supplemented by the possibility of seeing and feeling the structures which are at fault, in an open peritoneum, and without added risk to the patient; and the advantages of early and intelligent treatment by operation have been bestowed with a free hand by the same great agency.

But while I claim this for the recent practice of operation, I am of opinion that the tide of favour which has set so strongly in the direction of the early use of the knife has, in the particular class of cases with which I now propose to deal, risen rather beyond high-water mark, and we may presently expect something of a subsidence. This occurs with most new procedures, and is wholesome because necessary—a natural adjustment to the mean level which follows the rise and fall of opinion and practice.

The class of cases is that in which the obstruction is due to the accumulation of fæces, or flatus, or both, in the intestine—the condition known as ileus paralyticus It will at once appeal to the experience of surgeons when it is said that such cases are the most difficult we have to deal with, and the most unfavourable in their results when operated on. The diagnosis is a matter of the most extreme uncertainty; the seat of obstruction is often indefinite, and the great meteorism, and the long delay which has

so often taken place while the case has been neglected or ill-treated before it presents itself for surgical advice, renders the duty of the surgeon most difficult and the result of his labours very often unfavourable. Who, that has a large experience, does not know the difficulty of forming a judgment as to the time when operation should be undertaken, the anxiety attending the selection of a particular procedure, and the danger and impediment offered during operation by the protrusion of large masses of distended intestine—and who, if he confesses it, does not know the fatality attending laparotomy in such instances? And yet laparotomy in these cases is, as a rule, the only open-eyed and logical proceeding when minor methods have failed.

While my remarks will have reference to conditions which may forbid operation, or to methods which may render it unnecessary, I wish to guard myself against being supposed to be an opponent of the early and free use of the knife in suitable instances, and to state definitely my opinion in favour of timely operation. My position is simply this—that in the *furor operandi*, which has attacked so many of us of late years, we have been too prone to neglect or overlook methods of treatment which are comparatively safe and may be thoroughly efficient, and to subject patients to the risk of a laparotomy or other operation which might be avoided by less perilous treatment. There can be no doubt as to the dangers of timid delay, but the longer we observe cases of intramural obstruction the more we must be impressed with the difficulty and uncertainty of pronouncing a particular instance to be one in which delay should not be made, or in which treatment subordinate to operation might not be safely or successfully followed. There are no cases in which the golden "I have seen" is of more value, or which more forcibly argue that "all is but lip-wisdom which wants experience." We see one case which appears hopeless get well with simple treatment, and another about which our alarms are not excited die rapidly. This uncertainty in the prognosis of intestinal obstruction is a very grave difficulty, even when contrasted with the most uncertain of other complaints, and is one which indicates prompt treatment no less than cautious opinion.

There is a condition which may be called "intestinal shock" which is dependent on sudden obstruction, and is capable of producing rapid death without any assignable physical cause for dissolution beyond the indefinite and corporeally unrecognisable one called "shock." I will never forget the case in point of a vigorous young man who died in the Richmond Hospital two years ago. He was the subject of a congenital rupture; it became suddenly irreducible. On the day of his admission he had no symptoms of urgency; there was no tension of the hernia; he had presented himself at hospital not because he was in distress, but because he was uneasy at his inability to reduce the rupture. His bowel appeared, to use an old term, to be "incarcerated." Operation was deferred until the next day, as his symptoms were so slight that it was not thought to be called for or even justified. He became collapsed in the night, and in the morning he was dead. His body was, with the exception of the rupture, perfectly healthy; the imprisoned bowel was not even congested deeply; he had died of "intestinal shock." With such memories and such opinions it would be difficult to argue for delay as a general practice, but unquestionably it may, as an exceptional one, be followed with advantage in cases of ileus paralyticus.

The minor methods of treatment, which it is my purpose to discuss, are as follows :—

1. Local applications.
2. Opium.
3. Pressure and massage.
4. Purgatives.
5. Belladonna.
6. Tapping.
7. The enema.

As regards the value of these means, it is generally immaterial as to what is the sequence of clinical events in the particular case you have to deal with. The immediate condition is a bowel distended by fæces and flatus, and whose walls are too paralysed by over-distension to make any effectual efforts at expulsion, unless assisted by artificial means. It is true that the

collection in the lumen of the bowel is often due to a weak and atonic condition of its muscular coats, and a consequent failure of due peristaltic action. But whether the atony is primary and a cause of the collection, or secondary and consequent upon the presence of fæcal and flatulent accumulation, is not a matter which materially affects the treatment. The importance of succeeding in relieving obstruction by the use of one of these means is too evident to require argument, and will, at all events, appeal to the judgment of those who believe that laparotomy in intramural obstruction with much meteorism is an extremely fatal proceeding. Such cases are seldom seen by the surgeon until late in their history, when operation is very dangerous, and measures such as have been referred to as minor become major in their significance. So far as these minor treatments are concerned, there is nothing to be said of them in connection with their application to cases where the use of the knife may be resorted to that does not apply to them, but more strongly in those instances where operation is dangerous or unfit. The latter examples of obstruction, when intramural in character, are rendered more formidable by the presence of extreme tympanitis or obesity, and operation placed among such unfavourable surroundings that added importance is given to any measure which may help to avoid it. What, for instance, can be more embarrassing to surgical effort than to be forced to perform a laparotomy on a very obese or tympanitic subject; because, owing to the very conditions which render the operation unfavourable, the uncertainty of diagnosis forbids an extra-peritoneal operation, while the intraperitoneal one, so admirable under other circumstances, becomes extremely fatal where there is great distension of the bowel.

1. *Local Applications.*—Experience teaches the avoidance of such local medication as is capable of producing irritation. Turpentine, mustard, and other rubefacients applied to the skin are productive of little or no benefit, and are instruments of harm. They inflame the skin, increase the suffering they are intended to allay, mask signs in deeper parts, and place the tissues in the worst possible condition for operation if such be required.

A plain hot-water application, or simple poultice, can do no harm, and may be useful in relieving pain and stimulating peristaltic action. Beyond such means it is seldom advisable to go.

2. *Opium.*—Concerning the use of this drug in acute obstruction, I have nothing to say that is favourable. None of the advantages which attend its employment in many cases of chronic constipation seem to follow its use here. There is not much difficulty in understanding this if we remember that its chief therapeutic effect in some cases of constipation is due to its action in relaxing spasm—a physiological effect of little value in acute cases.

Half the cases of acute obstruction which come under surgical care have been drenched with opium, and, as a rule, with no effect beyond relieving pain, at the cost of masking symptoms and reducing the patient to a condition which, by adding to the difficulties of diagnosis, materially increases his danger. While it would be extreme to say that opium should never be employed in these cases, it is safe to assert that it is more often productive of harm than good, and that its general omission would be less hurtful than its usual employment.

3. *Pressure and Massage.*—In the very early stages of obstruction, and where the condition is due to the paralysis dependent on flatulence, or to the presence of fæcal accumulation, the support of a well-applied binder is of service. It often gives relief from pain, and, by supporting the bowel, prevents greater distension, and stimulates its efforts to contract. Massage carefully employed to the abdomen is a useful agent in assisting pressure, and is a powerful means of stimulating the natural efforts of the gut to rid itself of its load by contraction and peristalsis. Deep kneading and *tapotement*, alternating with friction in the direction of the colon, will be found most useful.

4. *Purgatives.*—I think we are too much afraid of using purgatives in the early stages of obstruction. Where the obstruction is a paralytic one, due to flatus or fæces, the propriety of employing them cannot be denied; and, even where the coats of the bowel are surgically engaged, their danger is over-rated. The use of saline purgatives in the treatment of peritonitis is a matter

in point, and shows how much undue fear formerly existed as to motion, even in those extreme abdominal cases where inflammation existed. Where they fail to do good, their power of evil is over-estimated. The moderate mechanical effect produced by even powerful cathartics is so comparatively slight that it need not be much dreaded, particularly when the resource of operation lies conveniently behind. The selection of a particular purge is one about which every surgeon will please himself. Personally I have been led by my colleague, Dr. Gordon, to confide most frequently in calomel, administered in repeated doses, and given either with or without the belladonna.

5. *Belladonna* is a drug which is not sufficiently valued in these cases. Its action is understood, and its administration logical. It relieves pain, while it does its work. It finds its chief use in those cases of peristaltic paralysis due to tympanitis or fæcal accumulation, and, as these are most often seen in very obese persons, and are attended by great meteorism—both conditions which render operation extremely dangerous—this drug is often an alternative with the knife. It should be given in full and repeated doses, and pressed until it shows its constitutional effect by producing either dilated pupil or dryness of the throat. The latter symptom is that most usually seen. It is perfectly useless employing this drug unless it be administered to its full extent. It may generally be combined with calomel. It is remarkable that whereas the tolerance of belladonna when given in chronic obstruction is very variable and its employment requires caution, it is extremely well borne in acute cases. The weak and impeded heart which is present in so many cases of acute obstruction is materially aided by the use of belladonna, which is a most excellent cardiac stimulant. In illustration I may refer to two recent cases, in both of which a most extreme condition of danger from obstruction was present, in both of which operation would have been most probably fatal, and in both of which belladonna was absolutely successful.

The first case was that of a gentleman whom I attended last January, in consultation with Dr. Neary, of Howth. He was a

man of middle age, of great obesity, with a large amount of abdominal fat, and with a weak heart. He was a gross feeder on rich and strong food. His attack had followed on an enormous meal of roast pork, and was evidently due to fæcal accumulation.

When he came under observation he was cyanotic, with a weak, rapid, irregular pulse, extreme dyspnœa, congestion in the bases of both lungs, temperature 101·2°, scanty secretion of urine, enormous distension of abdomen, and a general appearance of impending death. It was a case in which I would not have performed a capital operation, as I was sure it would have proved fatal. He had been treated by moderate purgation, enemata, and other appropriate measures, before I saw him, but the slight discharge which resulted had been productive of no benefit. He was ordered $\frac{1}{2}$ gr. ext. of belladonna and 1 gr. of calomel every third hour. In about thirty hours his bowels began to act and continued to discharge vast quantities of fæces and flatus until he was convalescent. In two days he took nine pills of the above composition, and six of half the strength, so that he received in forty-eight hours 12 grs. of calomel and 6 of ext. of belladonna. In this case the effect of the belladonna was as marked on the heart as on the muscular coats of the intestine.

The second case was one I saw lately with Dr. J. Kenny in the Hospital of the North Dublin Union. The patient was a spare, gaunt, middle-aged man, who had been without any action of his bowels for twelve days. The attack had come on suddenly, and had some resemblance in its mode of incursion to intussusception. We, however, came to the conclusion that it was a case of ileus paralyticus. All previous treatment had failed to give relief. The distension was of the most extreme character, and attended by vomiting. As it was impossible to localise the obstruction, laparotomy was the only operation that could be entertained, and as from the meteorism it promised to be one of very great danger, we resolved to give the man the chance of further medical treatment, particularly in view of the fact that he was suffering comparatively little distress. He got $\frac{1}{2}$ gr. each of ext. belladonna and calomel every third hour, and in twenty-four hours his bowels were acting copiously, and he made a rapid recovery.

6. The next treatment is *Tapping*. In cases of paralytic distension, where there is great meteorism and apparently not much

fæcal accumulation, it is indicated. It is most often justified, and most usually successful in spare persons, in whom the outlines of the distended intestines can be seen. The transverse colon is the part generally indicated for puncture, and the best rule about the use of the trochar is, if possible, not to precede its employment by purgatives. For if the bowel is filled with thin fæcal matter as a result of the ineffectual use of these drugs, the cannula becomes clogged, the escape of gas impeded, and the operation rendered of doubtful value. It is wonderful how small an escape of flatus will permit enough contraction of the bowel to induce action of its muscular coats, and give the fillip which produces a healthy peristaltic action. I have generally employed a cannula with a circumference of 5 mm., and furnished with a blunt stylet to remove obstruction if it should become clogged subsequent to its introduction. One case in illustration :—

In January, 1888, I saw, in consultation with Dr. Alexander Patton and Dr. Quinan, an elderly, insane gentleman, suffering from extreme abdominal distension and constipation. As usual in the cases of insane persons no account of his condition could be given by the patient, and no localisation of the obstruction made out. The condition was extreme, the only major operation admissible owing to the uncertain nature of the case was laparotomy.

The mental condition of the gentleman, and the great tympanitis, were arguments against opening his peritoneum; and as other treatment had failed, as the case seemed one of paralytic distension, and as his state was urgent, we decided to tap his transverse colon, which could be seen through his attenuated abdominal wall. A quantity of flatus and a little fluid fæces escaped, his bowel soon showed a disposition to undertake its normal contractions, substantial motions were passed, and a rapid recovery was made.

As between the employment of belladonna and of the cannula, *cæteris paribus*, the use of the needle is generally indicated in spare persons, and where flatus predominates over fæces; while in obese persons, in whom the intestine is deep and difficult to localise, or where much fæcal matter is present, the needle is not likely to be of service, and belladonna may do much good.

7. *The Enema.*—As Dublin is specially credited with the intro-

duction of a modification of the injection-tube, which has enjoyed a long repute, it is desirable that she should not be backward in the investigation of its real value, on which a doubt may be thrown. The use of the "long tube" was first proposed by Mr. James O'Beirne, one of the surgeons to the Richmond Hospital in the year 1833, when he published a book entitled "New Views of the process of Defecation," which he dedicated with extraordinary infelicity to the then Lord Lieutenant. This book is a model of all a book should not be. Tedious, clumsy, and discursive in style, over-loaded with ill-considered and unproven conjectures, it is, from its fulsome dedication to its windy appendix, a tiresome and over-rated production. That it has been so long accepted, and its teaching endured, is unaccountable. At all events a false god has been set up for professional adoration; it is called the "long tube," and its rubric is Mr. O'Beirne's treatise.

Let us see what he teaches; from a medley of impossible physiology and clumsy pathology, we may select a few of his propositions for consideration:—(1) That a tube 18 inches long can be passed through the rectum and sigmoid flexure, into the descending or even the transverse colon; (2) that considerable force may be used, because the bowel is so muscular that it is not likely to sustain rupture; (3) that the principal obstruction is likely to be the tight closure of the bowel owing to the contraction of its muscular fibres—that when you have pushed the tube in for 8 or 9 inches and find resistance suddenly give way, you have slipped through this colic embrace; and (4) that if the tube gets too soft with the warmth of the bowel you are to stiffen it by plunging it in cold water.

Now let us examine these few points *seriatim*. Can a tube 18 inches long be passed into the bowel without turning on itself? I do not believe the tube goes through the sigmoid flexure. From observations made not only on the living but on the dead body, I am convinced that if such an extraordinary chance as the passage of a tube through the sigmoid flexure ever does occur it is but the one exception in ten thousand which proves the rule. By careful pumping, fluid can be made to ascend to almost any distance into the colon,

particularly if gravity be called to aid by the adoption of the knee-and-head, knee-and-elbow, or lateral abdominal positions, and on this point O'Beirne's advice is sound enough; but when more than 8 or 10 inches of tube pass the anus, either a flexion has taken place, or the sigmoid flexure is pushed before the tube. These doubts are not singular. Mr. Treves, who has given the subject consideration, expresses himself strongly to the same effect, and relates a case in which a surgeon, having triumphantly demonstrated that the tube must be in the transverse colon, from the position in which its end could be felt through the abdominal wall, subsequent *post-mortem* examination showed an imperious stricture of the sigmoid flexure. I have seen the same thing happen in cancerous occlusion of the rectum.

The umbilicus is the sun of a system into various parts of which the poetic fancies of surgeons have carried the end of the tube. As the last-named writer says:—"It is of interest to note that many surgeons still cling, in a spirit of simple and trustful faith, to the 'long tube.' They appear to regard it as a kind of fetish, as an inanimate thing to be invoked as a last resource when in dire distress. History tells us that this remarkable pipe has been passed many feet into the colon, that it has reached the transverse colon, and that it has even found its way into the cæcum. I can only say that the adventures of this tube and the anatomy of the colon do not agree; there is a discrepancy somewhere, and, of course, it may be that the bowel is at fault. After many patient trials upon many dead bodies, I have not yet succeeded in passing this interesting pipe beyond the sigmoid flexure. Several feet of it have disappeared within the anus, and its tip has been felt about the umbilicus, and even the right iliac region, but the mobile sigmoid flexure has explained it all, and beyond that loop the good tube has not passed."

Second of O'Beirne's propositions: Is it true that the bowel is so muscular that considerable force may safely be used in introducing the tube? It is a dangerous theory, and has done infinite harm. If the bowel is not more often actually ruptured by vigorous disciples of Mr. O'Beirne it is because our bountiful mother Nature generally

orders that the tube shall turn against the pelvic wall, where it may bruise the unhappy gut against the bone but cannot completely penetrate it. No force is permissible, safe, or even useful in introducing an enema pipe. By judicious pumping, and by the due regard to gravitation, the fluid can be introduced without dangerous force. It is appalling to think of the risks run, and the damage done by the muscular apostles of the long tube. Life is sometimes sacrificed in this way by a rupture of the bowel, and it is to be wished either that O'Beirne had never written his dangerous book, or that some of his followers could learn a little anatomy and a good deal of caution.

Third: Is the principal obstruction, with the exception of impacted fæces, likely to be the tight embrace of a tube half an inch in diameter, by the muscular coat of the sigmoid flexure? Does any educated anatomist who has studied the intestine believe that this portion of a healthy bowel is capable of any such closure? Is the sudden disappearance of the resistance when the tube is in for 8 or 9 inches, due, as O'Beirne says, to the pipe slipping through this colic embrace; is it not more likely due to either the knuckling of the tube on itself, or to its end slipping over the brim of the pelvis, and carrying the gut before it.

The fourth of Mr. O'Beirne's propositions—namely, that if the tube is too soft it is to be hardened by immersion in cold water—is of a piece with the others, and will cry its own condemnation without any criticism.

In fine, the long tube is a mere convenience; it is easy to use, and clean in its application, but there is no good to be got in the way of introducing fluid into the bowel that cannot be effected as well with a pipe 8 or 10 inches long, while the latter is safer in unskilful or ignorant hands. The only good thing to be said for O'Beirne's book is, that although it taught much in error, it did some service in teaching, however inaccurately, that the injection pipe was an important instrument and one that should not always be relegated to nurses, but employed by the most educated hands.

I have no desire to exercise iconoclastic propensities by upsetting the traditions of a respectable antiquity, but it is a duty to speak

strongly in condemnation of this fetish, because it is an engine of extreme danger, and one whose power of evil cannot be too soon recognised by the younger generation. The sooner we disencumber our surgical minds of its false issues the better for our patients.

The enema is a most useful agent, and, when properly employed, a safe one. Let the material introduced by it be bulky, simple, and unirritating, let gravity and anatomical knowledge of the bowel be called to our aid, let tenderness supplant vigour, and intelligence replace length of tube, and we shall use the enema with more effect and with less danger.

The consideration of major operative measures are not within the compass of this communication, which deals with treatment antecedent to operation such as laparotomy or colotomy, suited for employment where these operations are forbidden. The points I desire to emphasise are these:—

1. That local treatment calculated to irritate the abdominal wall or produce rubefaction, is likely to do harm, and that only soothing and emollient applications should be employed.

2. That opium should not be generally or incautiously used, and that it is capable of being a most dangerous agent.

3. That pressure and massage may be of material value, particularly if employed early.

4. That while purgatives can only be sources of danger in cases where the obstruction is seated in or external to the wall of the bowel, they may be employed with freedom in intramural obstruction, and should, if tried, be used actively and for a short time only.

5. That belladonna, pushed to a full constitutional effect, is a most powerful agent.

6. That tapping the bowel in cases of meteorism is a comparatively safe proceeding; is capable of overcoming many cases of distension, and may be used more freely than at present, and—

7. That the full value of the enema can be got by the use of a short tube, that the long tube has obtained a credit it never deserved, and is, in incautious hands, a dangerous and sometimes a destructive agent.

If I have spoken truisms, attacked traditions, or insisted on trivialities, it is because the first are often forgotten, the second often too well remembered, and neglect of the last too often destructive.

SECTION OF OBSTETRICS.

SLOUGHING FIBROUS POLYPUS OF THE UTERUS.

By JOHN LILLY LANE, A.B., L.R.C.S., L. & L.M.K.Q.C.P.;
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[Read in the Section of Obstetrics, Nov. 23, 1888.]

It was not originally my intention to bring this case before the Section as a paper, but simply to exhibit the specimen. But being asked by some of the members of the Council to put it in the form of a paper on the grounds that it would be more likely to cause a discussion, and having on more than one occasion observed this to be correct, I have endeavoured to collect the history of the case as accurately as I could.

The patient is a lady, aged thirty-two, married three and a half years, and she was never pregnant.

The age at which menstruation commenced I was unable to ascertain, but I was told the periods lasted three days, quantity considered normal, and there was not any pain or discomfort experienced at the time.

In October, 1886, she consulted a gynæcologist, as she had been suffering for about a year (as I am informed) from a gradually increasing pale ichorous discharge, which caused much suffering owing to scalding of the external genitals and thighs. On the first visit this lady was told she was suffering from "ulceration of the mouth of the womb," which would be set right in a week or two. Her visits continued up to May, 1888, with some slight necessary interruptions, being seen once or twice each week.

Since May she has not been seen professionally by any medical man till I was called in.

Shortly after undergoing treatment in 1886, she suffered from irritability of the bladder, which she was told was in consequence of the ulceration, and her periods increased to a week—the quantity being proportionally increased.

In June, 1887, when the change was on, she got a wetting, and the following day a dull pain, as she expressed it, came on in the supra-pubic region, and since that time each menstrual flow has been accompanied by this same kind of pain, but more especially during the last two or three days of the flow.

Last January, without any cause, so far as the patient could make out, the change lasted for three weeks, and numerous clots were expelled. During that time the discharge for a day would occasionally be of a watery nature. Although run down considerably by the loss, she improved slightly till the following change came on, which also lasted for about three weeks; and, in fact, the change remained each time for three weeks. From January last till I saw her, except on one occasion—either in June or July—when it only lasted for a week, the interval between the changes was a fortnight. An unpleasant odour was first noticed accompanying the discharge in January, and this became worse with each successive change.

I was asked to see this lady on the night of the 11th inst. by her husband, but was unable, as I told him, to go then, as I was in attendance on a patient well advanced in labour, so he requested me to go as soon as I could, which I did early the following morning.

During my interview with the husband he informed me there was complete retention of urine, the supposed cause of which was cold caught during menstruation, but the chief thing that made me anxious about the case then was that the husband, who had been a medical student, and who had taken out practical midwifery in one of the lying-in hospitals in this city, said that he had passed a No. 8 gum elastic male catheter two or three times into the bladder, but no urine came. I questioned him closely as to the possibility

of his having failed to pass the catheter into the bladder, but that instead it might have slipped into the vagina, but he seemed very positive about it, and told me he had often passed a catheter when attending midwifery cases. And were it not that he said he thought there was urine in the bladder, both on account of the percussion note and the sensations of the patient, I would have been inclined to think it was a case of suppression rather than of retention of urine.

As I have already mentioned, I saw the lady on the morning of the 12th. The first thing I noticed on entering the room was the great fœtor, and next that the patient was evidently suffering intense pain though she was very uncomplaining.

The pulse was at the rate of 120 per minute, and the temperature was 103° F. In short, she was in a state of fever at the time.

I then ascertained she had had a red discharge for seventeen or eighteen days previously, and that she had not been able to pass urine from 10 o'clock the morning before; that she experienced a little difficulty on Saturday when micturating, but thinking that was caused by the bowels not having moved for some days, she took a dose of castor-oil on Saturday night, which operated three times, but she was unable to tell me if any urine passed at the same time or not.

On removing the bed-clothes to examine the abdomen the fœtor was something appalling, and I had grave apprehensions it was carcinoma. I was unable to make out anything by palpation owing to the tenderness of the abdomen and the thickness of its walls.

Before endeavouring to pass a catheter I washed out the vagina well with permanganate of potash solution, in order to remove as much of the discharge as possible so as to minimise the danger of carrying any of it into the bladder in the event of my succeeding in passing the instrument, and also to save my fingers from becoming contaminated.

Before taking the catheter I examined the vagina in order to ascertain what its condition was, and it did not take long to find out there was a tumour of some description in it, and that it was pressing on the urethra. I then washed out the vagina again, and

passed a glass catheter about the size of an ordinary No. 8. I found the orifice of the urethra right under the arch of the pubes, so that the tendency would be for the catheter to pass into the vagina; but, by sitting in front of the perinæum and placing the left index finger at the posterior border of the urethral orifice, I was enabled to guide the catheter to the orifice, and also to prevent it going into the vagina.

As soon as the catheter entered the urethra, I ran two fingers of my left hand into the vagina in front of the tumour, which I raised off the urethra; then, separating my fingers, the instrument passed up between them into the bladder with ease, though causing the patient a little pain. A considerable quantity of urine came away, when, as was quite natural, instantaneous relief was experienced.

I then endeavoured to examine more thoroughly, but owing to the vagina being small, the tumour filling it up almost completely, and the perinæum being rather rigid, as also the abdomen being still too tender to permit of pressure, I was unable to satisfy myself perfectly. I believed it to be a polypus, however, there being nothing else that I could imagine it to be, as an inverted uterus was, I thought, out of the question. I carefully washed out the vagina once more, removing, as I did before, a quantity of blood, the fœtor not being quite so bad as at first. I told the husband my suspicion, but said I would like to examine under ether before giving a decided opinion.

That evening I again saw the patient, washed out the vagina with carbolic acid solution, and drew off the urine; and on that occasion I was able to feel a tumour in the abdomen reaching almost as high as the umbilicus, but situated to the left side of the mesial line. This I took to be the uterus enlarged and dislocated.

The following day Dr. Macan saw the patient with me, when we examined her under ether, and he agreed in my diagnosis.

It was afterwards decided to remove the tumour on the 15th, but the chief difficulty that presented itself was how to deliver it without lacerating the perinæum, and the idea first was to incise the vulva freely each side and afterwards stitch them up.

I inquired at Fannin's if they had any instrument for reducing

the size of such tumours, but they had not. So then I made up my mind to chance the vulsella and a short midwifery forceps. The patient having been placed on a table prepared for the purpose, Dr. Corley kindly administered the anæsthetic, and I am also much indebted to Dr. Macan for his valuable assistance during the operation.

The vagina having been washed out with carbolic acid solution, the posterior blade of a speculum was introduced, which exposed the tumour to view. I then seized hold of it with a vulsella and tried torsion, but it began to break down, so I then applied a wire ecraseur without much trouble, and cut through the pedicle as high up as I could. Having applied the forceps, I made traction (assisted by Dr. Macan, who made traction with the vulsella); but, when the tumour began to press on the perinæum, fears were again aroused that it would rupture, and at the same time the vulsella tore through the tumour. I continued the traction gently, and then passed two fingers into the rectum and assisted the forceps in delivering the tumour without, I am happy to say, injuring the perinæum in the least. I then washed out the vagina, seized hold of the cervix with an American forceps, drew down the uterus which I washed out with a Bozeman, and then examined it with my finger. Close to the fundus could be felt the site from which the tumour had grown. I used the curette freely, removing a large quantity of thickened mucous membrane, washed out again, and injected with equal parts of tincture and liniment of iodine.

The patient, I am happy to say, has made an uninterrupted recovery.

The temperature, which was, on the morning of the operation, 100·8 F., had fallen to 99·6 F. that evening, and has been practically normal since, the pulse also being normal. Urine was not passed by the patient till the morning of the 18th.

Before concluding, I would like to mention a couple of points I consider of interest in this case—firstly, the unusual cause which led to the discovery of the tumour; and, secondly, the seeming absence of increased pain when the tumour was being expelled from the uterus.

INFLAMMATION OF THE BREAST AND ITS TREATMENT BY ELASTIC PRESSURE.

By ANDREW J. HORNE, F.K.Q.C.P.;
Late Assistant-Physician to the Rotunda Hospital.

[Read in the Section of Obstetrics, November 23, 1888.]

THERE are few practitioners of medicine who, from time to time, are not consulted occasionally with respect to conditions of the breast, yet the simpler though not less important affections more frequently come under the notice of the obstetrician, being often connected with some disturbance of lactation. I need not, therefore, make any apology for having selected mammary inflammation as a subject worthy of discussion before the Obstetrical Section of the Royal Academy of Medicine in Ireland. I have been actuated by considerations which, to my mind at least, seemed satisfactory. These considerations are the practical importance of the subject, the length of time since it has been discussed amongst us, as it is now nigh thirty years since that erudite observer, the late Dr. M'Clintock, read his classical paper on "Mammary Inflammation and Abscess" before the Surgical Society, and finally to elicit from the members present their opinions on the method of treatment to be pursued, as I have been led both by experience and reflection to consider that the methods usually taught and practised are gravely defective.

At the outset I wish to state that I do not claim any originality for the method of treatment I now advocate, as treatment by compression has long been recognised as recommended by Baron Scutin, O'Farrell, Scanzoni, and others too numerous to mention; but to Dr. Kelly, now of New York city, am I indebted for first bringing under my notice the treatment of mammary inflammation by elastic pressure, and more recently to a paper of Dr. Harris, of

Patterson, New Jersey, entitled "The Treatment of Mastitis by Bandaging and Rest." During my time as Assistant Master at the Rotunda Hospital, ample opportunity was afforded me of testing the value of this treatment, and so convinced was I of its utility that ever since I have adopted it in my private practice, with the most gratifying results—as a suppurating breast, I consider, ought to be a very rare occurrence, and will seldom occur in the experience of anyone who will apply continual and equable pressure, and will refrain from rubbing or using other artificial means, such as the breast pump. How much at variance this statement is when compared with M'Clintock, when he says:—"The resolvent treatment of mammary inflammation is admitted to be very unsatisfactory, rarely effecting the desired end, even though undertaken at the very onset of the disease, and carried out with vigour. In our most recent surgical text-books the treatment recommended is: In the first place, repressive measures should be employed, cold lotions, or the extract of belladonna, occasionally leeches and derivative aperients. Suckling may be continued, but it will be often necessary to use the breast-pump. Established inflammation, with redness of skin, and almost inevitable abscess, must be met with warmth and moisture, poultices, supporting breast in a sling, &c."

Mastitis is ordinarily observed only at the epoch of the functional activity of the mammary gland. When it is met with out of the puerperal period, it then coincides with certain periods of development of this organ—as, for example, in the newly-born, at the epoch of puberty, of menstruation, &c. When it occurs during the puerperal period, it generally makes its appearance about the second or third week, and, *en passant*, I may here remark, that except in the nursing mother, or those that have had the infant to the breast, have I ever known inflammation of the breast to follow. In women who have borne dead children, or whose infants perished during delivery, one would naturally expect inflammation more likely to follow; but experience teaches us otherwise. The explanation will easily be recognised when we consider for a few moments the etiology of the affection.

Etiology.—On the second or third day after labour, with the turgescence and increased size of the breast, the full functional activity of lactation is entered upon. This normal physiological incident was formerly regarded as most potent in the production of the various local and general disturbances so frequent at this time, and as the first symptom of a developing mastitis. As mastitis occurs almost exclusively only during lactation, it was quite natural to suppose that some irregularity in the secretion of the milk was the cause of the inflammation. Retention or impaired flow of milk played, therefore, an important part in the etiology of the affection. That retention of milk may, in the same way as the retention of other secretory fluids from accumulation of contents, cause, eventually, a slight circumscribed inflammation; but this is rare and totally different from the phlegmonous inflammation found in mastitis. Cold, mental disturbances, general debility, and even epidemic influences have been accused of causing mastitis, but I think it is now generally conceded that in all cases of mammary inflammation we find sore nipples have either preceded or are associated with the inflammation. The fissures and abrasions are the channels by which the infecting matter gains entrance and causes inflammation. The fact that sore nipple was a forerunner of mastitis did not escape the notice of M'Clintock, as he says—"A very large proportion of his patients had some form of sore nipple previously to the occurrence of inflammation of the gland; on a few occasions I have had an opportunity of observing the inflammation of the breast to supervene immediately and directly upon the ulceration or fissure of the nipple, as though it were an extension from it. The great danger to be apprehended from sore nipples is mammary inflammation." Later on he says—"Let it not be supposed that I regard abrasion or fissure of the nipple as the only cause of mastitis. Far from it, but I believe it to be a very frequent and influential one."

When inflammation has attacked the breast, it is generally indicated by pain, tenderness over a limited area, together with more or less febrile disturbances, rise of pulse and temperature, very often preceded by a rigor. Movement of the breast is productive

of pain, and a hardness is perceptible in some part of it, and over this a flush often exists on the surface. The indications for treatment, to my mind, are at once self-evident; secure the most perfect rest for the inflamed organ, let nursing from the affected side be discontinued, and, finally, let me sum up, in Dr. Harris's words (*loc. cit.*), rest from passive motion, rest from secretion, and rest from pain. All these conditions can in a great degree be immediately secured for the patient.

For the relief of pain, as also for its absorbent properties, I paint the affected breast with a five per cent. solution of oleate of mercury and morphin. I say five per cent., as the stronger solution may cause vesication. Place your patient in the sitting posture, and remove all clothes from the arm and chest, so as to allow the night-dress to fall about the waist. Envelop the affected breast in a layer of cotton wool, and begin to apply the bandage by gently raising the affected gland, first bringing it around the waist and securing it with a safety pin at the side of the dorsal vertebræ, then gradually encircling the breast, using as much pressure as the patient can conveniently bear, until the whole of the affected gland is covered. When thus applied, the patient at once experiences relief from pain, as the engorgement produced by the weight and hanging breast is at once overcome. Should the bandage exert undue pressure on any particular spot, the patient, by turning up a fold, can at once give herself relief without undoing the pins.

The infant can suckle the unaffected breast as usual. Should pain continue, I leave a prescription for an anodyne draught, but seldom is it necessary. I restrict the quantity of drink and fluid nourishment. In case the bowels are confined, administer a saline aperient.

I will not weary the Section with a recital of a number of clinical histories, but will select two of my more recent cases in which I adopted this treatment:—

Case I.—Mrs. C., aged 30, confined of her fourth child, May 27. Her labour was natural. On the day following delivery her breast filled rapidly. The child, a strong male infant, was applied

regularly. On the third day there was marked tenderness of left nipple, for which boracic lotion was ordered. The patient sat up on the tenth day, when my attendance ceased. On the seventeenth day this lady took a drive. At 11 p.m. on that night she felt cold and complained of great pain in the left breast, which was very much distended, red, and tender. The thermometer registered 103·5°; pulse, 120. I at once applied the bandage, and ordered an anodyne, in case the pain continued.

June 15th.—Temperature, 99·4°; pulse, 88. Patient had a fair night, but was troubled much with perspiration about head and neck, but had very little pain in breast. Slept after draught. The bandage was removed; patient immediately experienced pain, as the breasts were still very much enlarged, and asked to have it re-applied, as it gave such support. She did not wish to continue nursing any longer, so the infant was placed on a bottle. The bandage was re-applied daily for a week, as the milk flowed freely from the nipple, saturating the cotton wadding, at the end of which time the patient was allowed up; the bandage was then removed, but for some time subsequently the left breast remained much larger than the right.

Case II.—Mrs. X., aged 20, primapara, anæmic. On the eleventh day subsequent to delivery I was hurriedly sent for to see her. She was suffering much pain in the left breast. On my arrival I found the nurse had applied a large linseed poultice. The breast was consequently very red, and very tender to pressure. I at once removed the poultice and applied the bandage, but passed it over the right shoulder so as to leave the right breast free, as she continued to nurse. Two days subsequently the right breast showed symptoms of inflammation, so I included it in the bandage, and allowed the infant no longer to derive nourishment from the mother. It was re-applied daily until the fourth day, when the inflammatory symptoms had entirely subsided.

I regret very much I have not kept a complete record of all the cases in which I have employed this method of treatment during the past eight years; but only on two occasions have I had a breast go on to suppuration, and when this accident happened it was to a very limited extent, and the appearance of the patient was totally different from what I was wont to witness during my earlier experience with poultices, &c., and which every member

present has, no doubt, seen. It occasionally happens that a patient does not present herself for treatment until deep fluctuation is already present. Under such circumstances we may afford the patient considerable relief by applying the bandage, but taking care not to use much compression, as otherwise we would only entail more suffering. In this way we will naturally lessen the congestion in the breast, and limit the extent of the abscess. When it becomes necessary to open an abscess, I would do so by Lister's method, and still use the elastic pressure over the dressing, as much more easily applied than the ordinary strapping.

I have arrived at the following conclusions from my experience in the management of the puerperal breast :—

I. Mastitis is rarely seen, except in patients who have suffered from fissured or crushed nipples, and is the result of infectious matter gaining entrance.

II. That, as a rule, the secretion of milk continues only while the natural stimulus, as nursing or other means, continue to be employed.

III. That the secretion of milk, either in the normal or inflammatory state, begins to abate when such stimulus is withdrawn, and will entirely cease after a week or two.

IV. That in all cases of threatened or inflamed breast, well-regulated pressure by means of elastic bandage should be applied, and no attempt made to nurse or withdraw the secretion until the entire subsidence of the inflammatory movement.

The advantages of the elastic bandage over an ordinary roller :—

I. It is easier of application.

II. The pressure more uniform.

III. Not so likely to slip.

IV. More comfortable to the patient, as requiring much less material.

V. Not necessary to apply over shoulders.

PROLAPSE OF THE UTERUS AND VAGINA.

By WILLIAM J. SMYLY, M.D., Univ. Dubl.; F.K.Q.C.P.

[Read in the Section of Obstetrics, December 21, 1888.]

In order to determine in any given case of prolapse what the appropriate treatment is to be, we must, in the first place, ascertain as exactly as possible what the departure from the normal in that case is. In order to do so satisfactorily we must be able to recognise what is normal. Here we are met with a difficulty at once, for an organ like the uterus which is in constant motion cannot be said to have any one normal position. But this difficulty is, after all, more theoretical than practical, for though, like the arm or the leg, the uterus may have many normal positions, yet there is no great difficulty in determining when it is in one that it is not so.

If we examine the plates in any modern work on gynæcology, we shall find that when the bladder and rectum are empty the uterus is represented with the fundus behind the pubes and the cervix on a level with the last bone of the sacrum, so that in the erect posture it lies almost horizontally. This position, regarded by gynæcologists as the normal one, is altogether different from that figured by anatomists. The latter place the organ far back in the pelvis, with its long axis vertical. The result of a long and interesting discussion, in which much ingenuity has been brought to bear upon the investigation, proves that both are correct, and that retroposition and descent are *post-mortem* changes.

The question as to how the uterus is maintained in position during life is one of great complexity and difficulty, but it may be said, in general terms, to be due to the intra-abdominal pressure and the resistance of the pelvic floor. A great increase of the former may overcome the latter, or a weakening of the latter may

render it unable to support the former. Fritsch illustrates this very well by taking a book between the hands, the book representing the uterus, the lower hand the pelvic floor, and the upper hand the intra-abdominal pressure. Now, so long as the supporting hand is strong to resist the upper one, the book is held more firmly by the pressure from above, but if it become weakened or paralysed the book will fall; so also greatly-increased pressure from above will force it downwards. In like manner the uterus may descend either from a weakening of its supports or a greatly-increased pressure from above. A well-marked example of the latter was admitted into the City of Dublin Hospital a short time ago. The abdomen was distended with an enormous quantity of ascitic fluid. The uterus prolapsed, and the vagina, distended with fluid, formed a tumour between her legs as large as a fœtal head. The woman assured me that previous to the distension of the abdomen she had never had any falling of the womb. Such cases are, I believe, very rare, but ovarian and other tumours or spinal deformity may so encroach upon the abdominal cavity as to lead to similar results.

In the large majority of cases, however, the condition depends, as Hegar has shown, either upon a weakening of the peritoneal attachments of the uterus or of the pelvic floor. This division is of the greatest practical importance, and explains two distinct forms of prolapse—that, namely, in which the uterus first comes down, inverting the vagina as it descends, and that form in which the vagina draws the uterus after it. These two forms differ not only in ætiology, but in the anatomical changes which they produce, and they require totally different treatment.

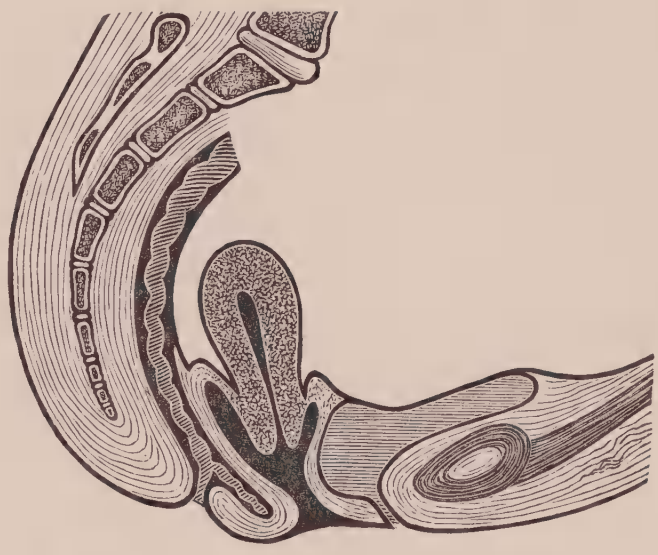
It is, therefore, of much importance to consider the arrangements by which the peritoneum and pelvic floor are enabled to resist the downward pressure of the abdomen. The pelvic peritoneum, owing to its wide surface of attachment, its elasticity, and the large amount of muscular fibres in its subareolar tissue, offers strong resistance to downward pressure, and its special arrangement in so-called ligaments enables it to place the organ to which it is attached in the most favourable position to resist this pressure.

If, for example, we consider the utero-sacral ligaments or folds of Douglas, their function is to draw the cervix backwards and upwards when from any cause it has been displaced in a contrary direction, and thus the fundus is thrown forward and the intra-abdominal pressure acting through the intestines falling upon the posterior or upper surface of the uterus, tends to increase its anteversion, and thus to diminish the angle between it and the vagina, and to close the latter. The round ligaments have a somewhat similar function, but it only comes into play when the organ is out of position; though not peritoneal, they share in the general relaxation, and need not therefore be considered separately. Relaxation of the pelvic peritoneum thus not only removes a strong barrier to descent, but the organ is allowed to tumble about and may assume a position very favourable to prolapse. In considering relaxation of the peritoneum we must not, however, confine our attention to the pelvic portion, for the entire peritoneum is, as a rule, relaxed, and is very frequently associated with floating kidney, pendulous abdomen, &c. This condition of relaxation may be a hereditary defect, and it has been observed in some families that prolapse in the female and hernia in the male members occurs with remarkable frequency. In by far the larger number of cases, however, this condition is the result of child-bearing.

Now as regards the pelvic floor, the vaginal slit is a source of weakness. It has been shown by Dr. Berry Hart that it divides the floor of the pelvis into two triangles—an anterior or displaceable and a posterior or supporting triangle. The posterior triangle is itself supported by the levator ani and coccygeus muscles. These muscles, to which the name of pelvic diaphragm was given by Luschka, arising from the pubes, pelvic fascia, and spine of the ischium, pass backwards and converge, to be inserted into the perinæum, rectum, and coccyx. The vagina is not only supported indirectly by these muscles, but is directly connected with the fascia covering them. When they contract, the perinæum is drawn upwards and forwards towards the pubes, causing a deep cleft between the nates, firmly closing the vagina and causing its axis to become more horizontal. If, however, they be relaxed or paralysed,

the posterior triangle falls backwards, allowing the vagina to gape and the pelvic floor to project.

Unfortunately relaxation of the peritoneum and of the pelvic floor are generally found associated, and the combined evils are well represented in the accompanying diagram. But in the wood-cut here we see that relaxation of the folds of Douglas has allowed the cervix to go forwards and the uterus to assume a perpendicular instead of a horizontal position; the posterior segment has been displaced backwards, and is followed by the anterior, which forms



a cystocle. The gaping vagina, it will be observed, is also perpendicular to the horizon, and its long axis therefore coincides with that of the uterus instead of forming an acute angle with it. In such a condition it is evident that the intra-abdominal pressure acting directly upon the fundus might drive the wedge-shaped organ into and through the vagina. If this did not occur the anterior vaginal wall, having lost its support, would probably continue to descend, and, dragging upon the cervix, would tend to draw down the uterus; the supra-vaginal portion of the cervix becoming elongated in proportion to the resistance offered by the ligaments, so that in some cases the fundus is found at the normal level, whilst the os appears in the vulva.

The foregoing is of course but a brief outline of the means by which the uterus is, during life, maintained in its position. There

are, in addition, some other factors, such as the retentive power of the abdomen, the rigidity and fixation of the vagina, and the adipose tissue of the pelvis, which time permits me only to mention; but still I think enough has been said to indicate the direction in which our therapeutic endeavours should tend.

Before proceeding to speak of cure, however, it may still be necessary once more to insist upon the importance of prevention. Labour and childbed are to be so conducted as to prevent, as far as possible, injury to the pelvic structures, to favour involution, and to guard against inflammation. The use of a well-fitting binder is also of importance in preventing pendulous abdomen.

In using the word cure with regard to prolapse, we do so in the great majority of cases in a comparative sense only, since a restoration of the structures to their original condition is possible in exceptional cases only. The various methods proposed for the cure of this condition, both operative and otherwise, are very numerous; but in the present paper I purpose dealing with those methods only which I have myself tried and proved. In a paper which I published some years ago in the *Dublin Journal of Medical Science*, I quoted with approbation the statement of Fritsch, "that it was rational to operate in every case of prolapse." Now, however, I would not go so far as that. Where, for example, the pelvic floor is uninjured, and the chief imperfection is peritoneal, the operative procedures which have been devised for the cure of such cases are so unsatisfactory that I think but few would resort to any of them so long as moderate comfort could be obtained from palliative measures; and if the results of massage prove as satisfactory as they have been reported to be, but few cases will be left for operation.

The treatment to be adopted in any given case will depend upon the pathological conditions which are present, and it is therefore, in the first place, necessary to make a correct diagnosis. For this purpose the patient should be placed in the dorsal position, with the perinæum facing a strong light, the thighs flexed and abducted, and the prolapse, if protruding, replaced. In the first place then the amount of pelvic floor projection should be noted, and if the

perinæum be drawn upwards by reflex action when touched. Two fingers are then passed into the vagina and the perinæum is drawn backwards to test its degree of relaxation. The margins of the levator ani muscles and pelvic fascia are sought for, and these muscles are carefully palpated on both sides. Next, the condition of the uterus should be made out, and especially the amount of cervical hypertrophy. The utero-sacral ligaments are then examined and are usually found to be very much thickened, and often tender to the touch. Lastly, the patient should be directed to cough, and the order in which the parts descend observed.

The first method of treatment to which I would direct attention is that by massage, or Thure Brandt's method. This is a subject which I approach with much diffidence, as I have not given it an extensive trial, but I believe that Dr. Smith has done so in the Rotunda Hospital, and I hope that ere long he will favour us with his experience. Without anticipating his remarks I may mention that this method consists of four separate procedures—1st, a *tapotement* of the spine; 2nd, massage of the pelvic contents; 3rd, lifting of the uterus—the organ being grasped by both hands through the abdominal wall; and, lastly, the forcible separation of the patient's knees whilst she endeavours to keep them in apposition. Upon the first and third of these I will not speak; but the value of massage in the cure of chronic inflammation of the uterus and its ligaments I have repeatedly proved, and that the forcible separation of the knees throws the levator ani into a state of firm contraction I have proved to myself and demonstrated to others.

In the large majority of cases we are still obliged to resort to the use of pessaries. I say "obliged," because I still believe that in any case in which a vaginal operation holds out a fair prospect of success it is preferable to the use of pessaries, which, by distending the vagina and causing leucorrhœa, tend to increase the mischief which they are intended to relieve; besides they condemn the patient to more or less constant medical supervision, which is in itself a great evil. The best pessary when available is Hodge's, or some modification of it, which by anteverting the uterus restores

the organ as nearly as possible to the normal position, but when the pelvic diaphragm is injured or paralysed such instruments are not retained, and we must then use broader ones, and that which I most frequently employ is Meyer's ring, which has the great advantage that the patient can remove and re-introduce it herself.

Of vaginal operations I have myself resorted to but two methods—namely, Hegar's and Martin's; the former in women who had passed the childbearing period, and the latter in younger women. I have done altogether eight such operations, and in one case only, so far as I am aware, has a recurrence taken place. That case I operated upon in 1885, and published in the paper to which I have already alluded. The patient was a midwife, and the prolapse complete, and both the late Sir Edward Sinclair, under whose care she had been, and I myself entirely failed in our endeavours to keep it up by any kind of support. The operation which I performed was Hegar's, and the result was, at first, very satisfactory. She resumed her occupation as a midwife and performed the most laborious duties, such as carrying her patients from one bed to another without inconvenience. After two years, however, the prolapse recurred. I again failed in the use of pessaries, and she herself tried various plugs, supported by T-bandages, but in vain, and she was quite unable to continue her occupation. It seemed to me that I had now but three courses open to me—1st, to remove the uterus; 2nd, to perform abdominal section and stitch the uterus to the abdominal wall; and 3rd, to shorten the round ligaments. I determined upon the latter, but being aware that experienced operators had, in some cases, altogether failed to find the ligaments, I resolved to practise, in the first instance, upon the dead subject, and for the opportunity of so doing I have to thank Professor Cunningham, who also afforded me the valuable assistance of Dr. Brooks. In the dead subject we found the ligaments and drew them out with great ease; but Dr. Brooks has since informed me that in some subjects which he has examined it was quite impossible to find them at the external ring.

In August, 1887, I operated on my patient. A thick layer of fat having been cut through, I found the ligaments without diffi-

culty following the guides indicated by Dr. Alexander, and drew them out several inches. I found more difficulty in drawing out the right ligament than the left one, and in my attempts to do so it gave way, so that I had some trouble in securing the stump and in stitching it to the pillars of the ring. The ligaments were secured as recommended by Dr. Alexander, the rest of the wounds stitched up from the bottom and dressed with Lister's gauze; nevertheless they suppurated, which somewhat delayed convalescence. A ring pessary was introduced into the vagina, which she still wears. Some time after leaving hospital a hernia formed in the right inguinal region, for which she wears a truss. This is, of course, a serious drawback, but I asked the patient herself if she regretted having submitted to the operation, and her reply was that the rupture did not prevent her going about and doing her business, whilst the prolapse confined her almost entirely to her bed.

In conclusion, I would state, as the result of my experience, that in all cases where the pelvic floor is incompetent I should resort to operation; but where the peritoneum is at fault I should use pessaries and reserve operative measures for those very exceptional cases in which they entirely fail to give relief.

AN ŒDEMATOUS FŒTUS, WITH ABSENT THORACIC DUCT.

By ALFRED J. SMITH, M.B., M.Ch., M.A.O., R.U.I.;
Assistant-Physician to the Rotunda Lying-in Hospital, Dublin.

[Read in the Section of Obstetrics, December 21, 1888.]

I VENTURE to bring before this Section of the Academy to-night a short paper on an œdematous fœtus, in the hope of inviting discussion on what is an extremely rare condition.

The specimen which I now show you was brought into the Rotunda Hospital by Mr. Taylor, on August 4th of this year. The mother, aged twenty-three, was an extern patient of the Rotunda Hospital, with a good family history. She had a healthy full-term child one year and seven months previously; her last menstruation was on March the 20th. The course of pregnancy was quite normal, until it was interrupted by a severe accidental hæmorrhage in the twentieth week; and it terminated on August the 4th in a premature delivery of twins, concerning one of which this paper is written. Nothing abnormal was observed with regard to the labour, the quantity of liquor amnii, or the placenta. One of the children, as you see, was quite healthy—it weighed three-quarters of a pound; length, ten inches, and I am informed made several attempts at respiration: the other weighed one pound; length, ten inches, and presented an extraordinary appearance; it looked like a mass of jelly. The head bore a curious resemblance to a large tomato; the pink transparent colour and the presence of frontal ridges made the resemblance singularly striking. The scalp was so distended that the bones could nowhere be felt, and the skin over the entire body seemed so tense that it was not possible to bend the limbs without the risk of bursting the skin. There was no pitting on pressure; on puncture a glairy fluid

resembling white of egg, escaped. The general appearance is fairly well shown by referring to the photograph, of which A shows the front, and B the lateral aspect of the fœtus. One observes the general plumpness of the body (the lower extremities have become completely shrunken by an accidental immersion in a strong alcoholic solution), the apparent complete absence of the eyes, and in the frontal region a distinct cleft running on either side upwards and outwards; it bifurcates about three-eighths of an inch from the nose into branches, one of which runs upwards and outwards, the other downwards and outwards, enclosing ridges, which suggest the undeveloped ocular vessels; and a fissure is also seen to run obliquely downward and inwards from the pinna of either ear.

The number of recorded cases of general œdema of the fœtus are very rare indeed. Prof. Delamotte's case in 1690 is, I think, the first, but interesting accounts are given by Betschler,[a] Steinwirker,[b] Lawson Tait,[c] and others. Up to the present no satisfactory explanation has been given as to the causation of the general œdema. Dareste,[d] while making experimental researches on the production of monsters, found, as a consequence of arrested development of the vascular spaces, that the blood corpuscles remain imprisoned in the cavities where they were formed, a general dropsy of the embryo follows, with a marked accumulation of fluid in the tube that forms the spinal cord and cephalic vessels; this was observed in chickens. Betschler[a] believes the œdema to be due to an obstacle to the circulation in the umbilical vein; Steinwirker[b] looks on it as elephantiasis congenita cystica, while Mr. Lawson Tait looks upon premature closure of the foramen ovale, which condition Dr. Sawyer found in his œdematous fœtus, as a predisposing cause for the universal œdema. All previous hypotheses appear to me untenable, the explanations given being for the most part speculative. I considered it most probable that the general œdema in this particular fœtus was due, as in the case of

[a] Breslauer Klin. B. 1. L. 260.

[b] D. i. Halle. 1872.

[c] Obst. Trans. 1875.

[d] L. Tarnier et P. Budin. 1886. Tome I., p. 354 et 355. Traite Accouchments.

hydrops occurring in the bovine species described by Prof. Franck, to an absence of the thoracic duct and mesenteric glands. This theory could only be satisfactorily established by a complete dissection, which Professor Birmingham, President of the Anatomical Section, kindly consented to undertake; and I am deeply indebted to him for the most thorough investigation of this subject, the result of which I have much pleasure in placing before you.

Anatomical Report.—The fœtus looked like two tomatoes strung together—one being represented by the head, the other by the body. The head was extremely large, and was made up of a number of puffy swellings, separated by deep sulci. On the anterior aspect were two bulgings on each side, in addition a large one *above*, extending from the nose over the head, and a small one below the chin. The nose, the mouth, and the chin were almost completely buried beneath these overhanging swellings. From the round pimple-like nose there ran outwards and upwards on each side a deep sulcus, which bifurcated about three-eighths of an inch from the nose into branches, one of which ran upwards and outwards, the other downwards and outwards. Another deep sulcus ran down from the side of the nose on each side close to the middle line, and parallel to its fellow of the opposite side, separating the mouth and chin from the distended cheeks. The whole appearance at first sight reminded one of the visceral arches and clefts; but these apparent clefts are simply natural depressions which are made sulci, sometimes half an inch in depth, by the enormous œdematous distension of the subcutaneous tissue in the intervals between the depressions. On the surface—even upon going to the bottom of the sulci—no trace of an eye or eyelid can be found. On dissection a well-developed eye was discovered underlying the point of bifurcation of the fissure, which runs upwards and outwards from the nose, not under the rounded prominence in the angle between its branches, as might be expected. The subcutaneous tissue was œdematous to such a marked degree that in parts it was three-fourths of an inch in depth, while the subcutaneous tissue in the healthy twin brother is only about one-fiftieth of an inch in thickness. The muscles and the other

tissues of the body, which contain much areolar tissue, are also very œdematous.

The organs of the body generally, as far as examined, are normal, with the exceptions mentioned below; the heart is rather smaller as compared with that of the healthy fœtus; foramen ovale patent; the thyroid gland seems to be normal.

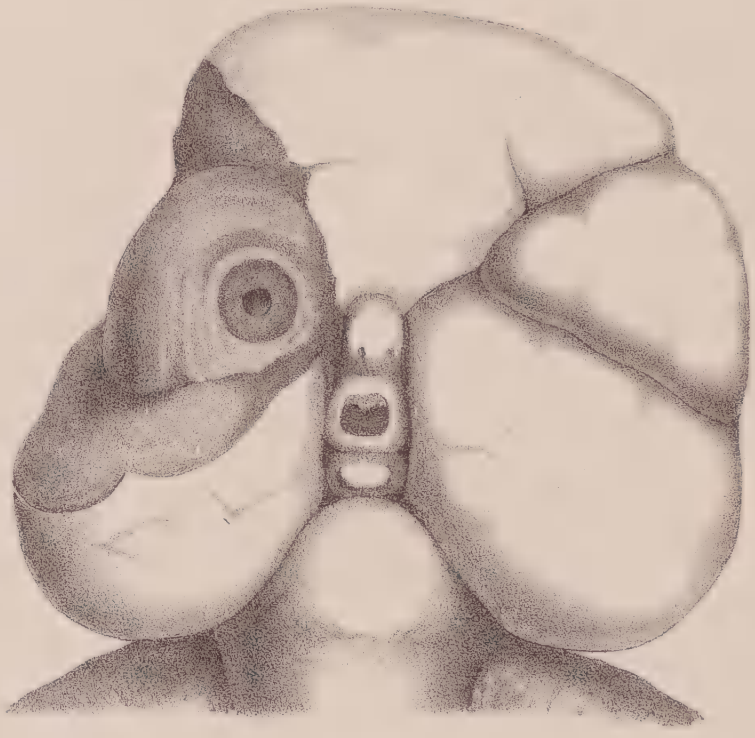
No trace of a thoracic duct could be found. I examined the fœtus with the special object of finding if this duct were present, and the greatest care was taken in examining the region in which it is generally found. I dissected the posterior mediastinum from the right side—no trace. I examined the junction of the jugular and subclavian veins carefully, with no result, both sides. I dissected the mediastinum from the left, examining that side of the vertebral column and aorta, and still with a negative result; then I went down along the aorta into the abdomen, and sought for the receptaculum chyli, with a similar result; so after a careful examination I was compelled to decide that there was no thoracic duct. Previously I had examined the healthy twin brother, and found the duct with the greatest ease in the normal situation.

Next I examined the mesentery for mesenteric glands—having previously found them present in large numbers and of considerable size in the healthy twin, with the naked eye; no glands could be made out. The mesentery is easily examined by holding it up to a bright light, when the slightest thickening is apparent, but no trace of a gland could be found by this means, even with a hand microscope, while, as stated, the glands are at once evident to the naked eye in the healthy brother.

Microscopic Examination.—To Dr. Earl, of Trinity College, my best thanks are due for the careful microscopic examination of portions of the specimen sent him. He writes:—The piece of skin examined shows very great thickening. In different parts of the sections, beginning at the deep surface and extending to the epidermis, dilated vessels are seen generally lying in smaller or larger groups. These are evidently lymphatic vessels, as some contain homogeneous masses of coagulated lymph. In the section stained in eosine the distinction of lymphatic and blood vessels

A B

C



D

A. Birmingham del. F. Huth, Lith^r Edin^r

ŒDEMA WITH ABSENT THORACIC DUCT.

is well seen on account of the fact that eosine stains red corpuscles.

In the piece of the mesentery examined I found *nothing* of the nature of a lymphatic gland.

Arguing from the reports just read, I consider I am justified in stating that the complete absence of the thoracic duct lead to a blocking up of the entire lymphatic system, and imprisoning of the normal transudation in the dilated lymphatic spaces lead to the universal œdema.

A similar condition of complete absence of the thoracic duct and mesenteric glands in the human species has, after a most diligent search, I believe, not been observed up to this.

The obstetrical bearing of an œdematous fœtus lies in the difficulty of diagnosing the presenting part, as is evidenced by the case reported by Mr. Lawson Tait, where a head presentation was mistaken for that of a breech; and that a general watery gelatinous infiltration of the subcutaneous tissue may lead to the gravest form of dystocia, is shown by the case described by Keith in the *Edin. Med. and Surg. Journal* for April, 1855.

EXPLANATION OF PLATE.

A. Portions of mesentery of normal fœtus, showing numerous mesenteric glands (natural size).

B. Portion of mesentery of œdematous fœtus, showing absence of mesentery glands (natural size).

C. Head of œdematous fœtus, showing extraordinary appearance of parts produced by the œdema. No trace of an eye can be fonnd on one side; it has been exposed by dissection upon the other (natural size).

D. Section of skin and superficial portion of subcutaneous tissue of œdematous fœtus, showing the enormously enlarged lymph-spaces, some of which are partly or completely filled with a colloid-looking material, apparently coagulated lymph (magnified).

ON AMPUTATION OF THE CERVIX UTERI IN THE TREATMENT OF UTERINE CANCER AND CERVICAL INJURIES.

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[Read in the Section of Obstetrics, February 1, 1889.]

Amputation of the cervix uteri appears to me one of the few gynæcological operations less frequently resorted to than might be justified by the advantages derivable from its employment in some cases in which far graver and not more successful operations are too often performed.

In the following observations I shall discuss briefly, from the standpoint of my own clinical experience as well as from the literature of the subject, the expediency of the timely removal of the cervix uteri in cases of threatened or early existing malignant disease. Secondly, I desire to again urge the utility of this operation as a substitute for cervical trachelorrhaphy in certain cases of laceration and its consequences. These two subjects are, to a large extent, so closely connected that they may be properly considered together in this communication.

Frequency of Cancer of Cervix.—Cancer of the uterus is not only the most formidable, but is also unfortunately amongst the more frequent of the special diseases to which women are subject. Thus, according to statistics with regard to the comparative frequency of uterine cancer cited by Dr. Duncan, of 26,200 women examined by Professor Schroeder, 812, or 3 per cent., had cancer of the uterus; in 236 of these the portio-vaginalis was attacked, in 181 the mucous membrane of the cervical canal, in 28 the body of the uterus, and in 367 the place of origin was undetermined. The

time of life most subject to the disease is that subsequent to the menopause. To this rule, however, many exceptions are found—one of the most remarkable instances of this kind that I have met with being the case of a girl only sixteen years of age, who died on the day after her admission to my ward from exhaustion, consequent on a large encephaloid cancerous mass, growing from the cervix, and completely occupying the upper third of the vagina.

Cancer of Uterus generally originates in the Cervix.—The chief points I desire to submit for discussion on this subject are—firstly, that cancer of the uterus, in the great majority of cases, originates as a local disease in the cervix, and if there recognised in time, and treated by immediate infra-vaginal amputation of the cervix, it may possibly be arrested. Secondly, that if not thus checked, the subsequent extirpation of the entire cancerous uterus, as advocated by some authorities, is an operation which, if not worse than useless, can seldom be resorted to with any great probability of ultimate success. And, thirdly, that in cases in which, as will be shown, there is a likelihood of cancer becoming subsequently developed in cicatricial tissue resulting from parturient lacerations, the cervix should be unhesitatingly removed as the only effectual preventive measure of a disease for which, when established, there exists no reliable curative treatment.

Within the past twelve years a considerable number of cases of uterine cancer, chiefly epithelioma, have come under my observation in the gynæcological department of the Mater Misericordiæ Hospital, and in the great majority of these the disease primarily manifested itself in the cervix, whence, if unchecked, it gradually extended upwards into the body of the uterus. During this time I have, however, also met with some cases in which the uterus was disorganised by epithelioma of the endometrium, or by extensive cancerous infiltration into the submucous structure, or was occupied by sarcomatous or other malignant growths, long before any cervical disease was apparent, as well as with cases of uterine cancer, in which, whilst the cervix remained unaffected, the fundus and body of the organ were disorganised by extension of malignant disease, probably ovarian or tubal in origin.

Dr. Emmet has clearly shown that cancer of the uterus frequently commences within the cicatricial tissues, resulting from neglected parturient lacerations of the cervix. Mr. Jonathan Huchinson points out that in many cases the first stage of cancer is that of inflammation; that "all inflammations are infective, and inflammatory processes may pass, by almost imperceptible gradation, into maglinancy." "In more than three hundred cases of cancer of the cervix, of which I have notes," observes Dr. Reamy, of Philadelphia, "but one occurred in a virgin, and but ten in married women, in whose cases I could obtain no evidence of abortion or child-bearing. Can anyone deny that such facts point strongly to the local origin of cancer in these cases at least? And can it be doubted that injuries inflicted upon the cervix during parturition have a direct or remote connection with this origin? Would it not be wise to repair these injuries, when they are appreciable, by whatever method it may be done most speedily and certainly." Many years earlier a somewhat similar observation was made by my distinguished teacher, the late Professor Montgomery, in a paper of his on the "Incipient Stages of Cancer of the Womb," in which he clearly pointed out that, as a general rule, the first discoverable morbid change, which is the forerunner of cancerous affections of the uterus, takes place in and around the muciparous glandulæ or vesicles, sometimes called ova Nabothi, which exist in such numbers in the cervix and margin of the os uteri; these become indurated by the disposition of scirrhous matter around them, and by the thickening of their coats. Rokitansky also long ago remarked that cancer of the uterus generally limits itself to the vaginal portion and cervix in a defined and sharp manner. And he further remarks:—"The primitive seat of cancer is always the cervix uteri, and first of all the vaginal portion. The primary appearance of cancer in the fundus uteri is limited to such extremely rare cases, that what we have just said remains a fixed rule."

In relation to the same question we must, moreover, as the late Sir James Simpson well said, bear in mind the natural history of carcinoma uteri; and in illustration of this he quotes Dr. Walshe's observation:—"Uterine cancer is commonly primary," and pos-

sessed of comparatively slight tendency to contaminate the system generally. In two important respects, therefore, uterine carcinoma presents conditions favourable for surgical interference; still, however, in order that a case may offer any chance of success, several conditions seem requisite:—

1. The disease must be in an early stage.

2. The morbid structure must be strictly limited to the lips or cervix, or, at all events, be distinctly situated below the line of reflection of the vagina upon the cervix uteri.

The above combination of circumstances is seldom met with because the medical attendant is rarely consulted until the disease has passed the limits in question. Nevertheless, of eight cases referred to in Simpson's monograph on this subject here cited, in which he thus amputated the cervix, seven recovered perfectly. The same view is supported by more modern authorities, and as Hart and Barbour point out, we may, in accordance with the researches of Klebs, Waldeyer, Virchow, and others, trace the origin of all these malignant growths either to the cervical epithelium of the cervical glands, the deepest layers of squamous epithelium on the vaginal aspect of the cervix, the connective-tissue cells of the cervix, or the epithelium of the cervical canal.

Symptoms of Uterine Cancer.—The constitutional symptoms of uterine cancer in its later stages need not be here enlarged upon, being general emaciation, sharp, pinched, anxious facial expression, yellow, semi-jaundiced discoloration of the whole surface, and the well-marked hectic and other unmistakable evidences of the cancerous cachexia, when once the disease is fully established. Nor, in this connection, is it necessary to dwell on the evidences revealed under the same circumstances by local examination when carcinoma had extended beyond the cervical zone, and when the characteristic pain, hæmorrhage, and discharge, as well as the symptoms caused by the pressure of the enlarged, misplaced, or adherent cancerous uterus, or consequent on the extension of the disease to adjacent parts, leave no room either for doubt as to the diagnosis, or for hope as to the prognosis of the case.

The early symptoms of malignant disease of the cervix uteri, and

the incipient progress thereof, are so obscure and ill-marked in their apparent consequences, and the frequency of the disease is such, that it behoves us to examine most carefully for any of those symptoms of its existence which, however unreliable and obscure *per se*, yet in their aggregate may afford the careful observer sufficient grounds on which to base his diagnosis in such cases.

In like manner, once the cervical disease has attained the ulcerative stage, there can be no question as to its detection, though then commonly but too late, when, on digital examination, the cervix is found lower than normal, the uterus being bound down in the pelvic cavity by adhesion to the surrounding tissues, the os open, its lips, if not already destroyed, tumefied, or else perhaps presenting the stony hardness characteristic of the scirrhus form of disease. The cervical structure is then more or less deeply excavated by the subsequent ulceration; occasionally the entire os and cervix being thus eaten away, so as to lay the uterine and vaginal cavities into one, before any suspicion of the nature of the case has called for examination. By the speculum the characteristic found—irregular, thickly corroded ulcer, covered with a gray viscid putrilage—now comes into view, whilst if any possible doubt could then exist, this would be cleared up by the presence of the peculiarly fœtid ichorous or watery discharge attendant on open cervical cancer.

Without dwelling further on these symptoms, what I would rather desire to impress here is the possibility of recognising the earliest incipient stage of cervical cancer long before any pain, hæmorrhage, or characteristic discharge is observable, on careful examination of the cervix, by evidences which were well described by the late Dr. Montgomery, but which are generally passed over without sufficient notice by later writers, as well as by microscopic examination. On careful exploration at an early period in such cases, as Dr. Montgomery pointed out, the margin of the os uteri is found hard, and often slightly fissured, and projects more than is usual, or than is natural, into the vagina, and is irregular in its form. In the situation of the muciparous glands, there are felt several small, hard, and distinctly defined projections, almost like grains of shot or gravel, under the mucous membrane. Pressure on these

with the point of the finger gives pain, and the patient often complains that it makes her stomach feel sick. The cervix is, in most instances, slightly enlarged, and harder than it ought to be. The circumference of the os uteri, especially between the projecting glandulæ, feels turgid, and to the eye presents a deep crimson colour, while the projecting points have sometimes a bluish hue.

In these cases, according to Professor Schuata, of Prag, it is possible to make an early diagnosis only by regarding every case of catarrh and erosion with suspicion, and by examining them very carefully as to whether we can discover somewhere a small nodule which must be regarded as cancerous. The age and clinical history of the patient, hæmorrhages, and inspection of the vaginal portion, will teach us that such erosion will show some suspicious points. Such points have the following peculiarities:—On the surface of the erosion we find a small nodule projecting which is dark red or yellowish red, bleeding very easily. These nodules seem, at first, quite innocent; nevertheless when excised they present, under the microscope, all the characteristic signs of cancer of the uterus. The future of this disease will be better if we operate early. This, however, depends on our early diagnosis. In every case, continues Schuata, will the excision of a suspicious nodule be necessary. This is quite a harmless and painless procedure.

Under these circumstances the diagnostic utility of the curette, as recently here well shown by Dr. Smyly, becomes unquestionable; and for this purpose its cautious employment, so as to bring away a sufficient amount of the abnormal tissue for microscopic examination, should immediately be resorted to in every case of suspected or possible adenomatous or cancerous degeneration.

Expediency of Early Amputation of a Cancerous Cervix and its Results.—The question of the expediency of operative interference in these cases, by removal of the cancerous cervix, must be determined on similar principles to those recognised in all other forms of malignant disease. Thus, for instance, no surgeon would, I think, be justified in amputating the cervix in cases where the cancerous cachexia was well established and obvious in the general aspect of the patient, or where the disease was either developing

with unusual force and rapidity, or else had extended beyond the cervical zone, so as to render it impossible to thus remove completely the implicated part.

In a large number of instances, however, the disease, if sufficiently early recognised, may, I believe, be completely and permanently arrested by the infra-vaginal amputation of the cervix by the écraseur and the free cauterisation of the stump, without any necessity for resorting either to Schroeder's more complicated flap operation or to the supra-vaginal method of accomplishing the amputation of the uterine neck adopted by the same distinguished authority, the results of which are not I think, such as should lightly lead us to induce our patients to submit to its risks, as in 13 out of 19 cases the disease recurred within four and a half months of its performance, whilst the mortality was 4 in 37.

At a meeting of the Obstetrical Society of Berlin, held October 23, 1886, Hofmeier reported the number and results of Schroeder's partial amputation of the uterus for cancer, and the results of total extirpation by the same operator. The report included all of the cases which had been operated upon from October 1, 1878, to October 1, 1885. During that period there were 118 partial amputations, with 10 deaths—mortality, 8·5 per cent.; and 48 total extirpations, with 12 deaths—mortality, 25 per cent.

In 1882 Pawlik published a Report of 136 cases of cancer of the cervix which had been treated in the first gynæcological clinic of Vienna by means of the galvano-cautery, the observations extending back to 1861. The mortality from the operation, although not exactly stated, must have been exceedingly small—apparently not over 3 or 4 per cent. By amputation with the hot iron, Schroeder, out of 13 cases, had an operation mortality of $7\frac{7}{10}$ per cent., and of the survivors 42 per cent. remained well at the end of eighteen months to two years. Carl Braun and Simpson cite instances of patients who were operated upon in this manner, and who had continued alive and well for a quarter of a century.

Four years ago this subject was very fully discussed in the London Obstetrical Society, where Dr. Godson exhibited specimens from cancerous cervices which he had amputated, and in which

there had been no recurrence of the disease after a lapse of six and three and a half years respectively after the operation.

According to Ruge and Veit, cancer of the portio vaginalis has a very slight tendency indeed to spread into the cervix, but almost invariably spreads out laterally into the fornices of the vagina and the ligaments. "What possible advantage then," asks Dr. Duncan, "can be expected in these cases by removing the uterine body (which is unaffected) over that gained by supra-vaginal amputation which gets rid (as far as the uterus is concerned) of the disease? On the other hand, cancer of the mucous membrane of the cervical canal spreads up along the canal and into the uterus, whilst it only at a late period (or even not at all) attacks the os uteri."

I need not here refer in detail to the cases in which I have myself removed the cervix, inasmuch as I have found it very difficult, as a general rule, to trace accurately the subsequent history of hospital patients once they are lost sight of, after their discharge from the wards. All I can say on this point—without at present here referring to some of my earlier successful cases—is, that within the past eight years I have amputated the cervix in eighteen cases for cancer, or what appeared to me to be incipient cancer; and that all the patients thus treated recovered from the immediate effects of the operation, and were discharged as convalescent. In four of them the disease recurred either in the uterus or elsewhere within a year; in two it returned within two years; in one instance the patient returned two and a half years subquently with a large cancerous ulcer of leg from which she died shortly afterwards; and in another it recurred two years later. Ten cases have continued, as far as I have learned, in good health.

Considering, therefore, the extreme liability to the occurrence of cancer in the vicinity of cervical lacerations, the difficulty of early recognition of the disease, and the great uncertainty of treatment by any operation after the disease has become evident, I would venture to urge the removal of the cervix in all cases of extensive areolar hyperplasia consequent on parturient laceration as a preventive of the possible development of uterine cancer. This rule of practice may be objected to as involving an unjustifiable and

meddlesome surgical interposition in cases where the disease might never occur were the patient left unmutilated by the removal of the cervix uteri. But though this may possibly have been the case in some of the instances in which I have performed the operation in question, I shall be content to bear that reproach with perfect equanimity so long as I feel satisfied, as I do, that by its adoption I have been instrumental in any instance in warding off a disease so dire as cancer, which, as before observed, clinical experience has proved most liable to attack the cervical tissues in the cases above referred to.

Method of Operating.—Amputation of the cervix may be accomplished by either the infra- or supra-vaginal methods, and of these, under ordinary circumstances, I much prefer the former. In its performance we may use either knife, scissors, or galvano-cautery, or ordinary chain or wire écraseur. In my own practice I have in most instances employed the latter with very satisfactory results, although its use was objected to by the late Dr. Marion Sims, on the grounds that it makes a lacerated surface to heal by granulation, which takes a long time, often leaving the os tineæ contracted. Another objection to it is the uncertainty of amputating just where we place the chain, which often draws in more tissue than we intend, and removes more than we wish. So great has been this trouble, that some of the German surgeons have given up the écraseur altogether in operations on the neck of the womb, because the attachment of the bladder and, in some instances, the posterior *cul-de-sac* of the vagina have been injured, and even the peritoneal cavity opened by its greedy grasp. I have not myself observed the occurrence of any of the mishaps thus referred to by Dr. Sims in my own experience of the use of the steel wire écraseur for amputation of the cervix uteri.

In amputation of the cervix, the patient being in the ordinary lateral semi-prone position, and the parts well exposed by the duck-bill speculum, the uterus should be so thoroughly drawn down by forcible traction with a strong vulsellum that the cervix protrudes well beyond the vulva, so as to elongate the structures and allow of the wire of the écraseur being applied as high above the

diseased portion as possible, and above this again the uterine neck should be transfixed by a stout wire ligature, so as to prevent subsequent retraction, and facilitate the full use of the cautery, which I regard as essential. The advantages following the use of the actual cautery after removal of the cervix for cancerous disease are, I believe, unquestionable, and may probably be due, as M. Sédillot has considered, to the creation thereby of an adventitious fibrous tissue of low vascularity, which is more or less refractory to morbid change, and which acts as a barrier to the recurrence of the disease.

Extirpation of the Entire Uterus for Cancer as an Alternative Operation.—In this connection it appears to me necessary to refer to the comparative advantages claimed for the only alternative operation proposed for cervical amputation in cases of cancer—namely, the entire extirpation of the cancerous uterus. In so doing I shall not attempt to discuss fully the respective merits or demerits of either the vaginal method, or the supra-vaginal or abdominal operation introduced by Dr. Freund, or any of the still more recent modifications of hysterectomy suggested for this purpose. I may, however, cite sufficient facts to justify the opinion that their immediate risks, as well as the very small percentage of cases in which any of these procedures can be resorted to with any prospect of permanent benefit, are sufficient to exclude them from the position of general operations of selection, even in a disease so fatal as that under consideration.

The high authority of Dr. Martin, of Berlin, is deservedly recognised on all gynæcological questions; nevertheless, I would venture to express my emphatic dissent from the conclusion he has formulated in a paper in the Transactions of the last International Medical Congress on this important subject, in which he says—"I recommend the vaginal extirpation of the uterus as the operation, as the means, which we ought to apply in cases of cancerous diseases of the uterus, as long as the disease is limited to the uterus itself." For my own part, bearing in view the enormous mortality supervening on operations for the extirpation of the cancerous uterus, whether performed by abdominal or vaginal

methods, as well as the common recurrence of the disease in those who survive the immediate consequences of such operations, I cannot regard their employment as generally expedient or justifiable.

An excellent *résumé* of the earlier literature of the vaginal operation for extirpation of the uterus, down to that date, was given by Dr. Condie nearly forty years ago, in the American edition of the late Dr. Churchill's work on "Diseases of Women," and from this we learn that, according to Boivin ("Diseases of the Uterus," p. 248), Palletta was one of the first, if not the first, who performed this operation without being aware that he had extirpated more than the cervix uteri. Since that time it has been performed once by Sauter, twice by Siebold, once by Holscher, four times by Blundell, once by Barnes, once by Lizars, three times by Recamier, thrice also by Langenbeck, once by M. Dubois, once by M. Delpech. Of all the nineteen patients, sixteen died in consequence of the operation, one as late as the fourteenth day (Langenbeck's), another on the fourth (Barnes's), most of them on the following, or third at the latest; some in a few hours, or even a few moments after the operation.

Dr. Blundell performed it four times; one case recovered, three died shortly after the operation. He remarks: "If cancer of the lip may be removed with success, I should be inclined to hope that the same success might attend extirpation of the malignant scirrhus of the uterus."—"(Diseases of Women," p. 180.)

Velpeau says that the operation has been performed twenty-one times in twenty years, and of all these not one has been permanently cured.—(*Méd Operatoire*, p. 162.)

In a valuable communication to the London Obstetrical Transactions, 1886, "On the Extirpation of the Entire Uterus" in certain cases of cancer already referred to, Dr. William Duncan has collected from various authorities the particulars of a large series of cases of these operations, of which 137 were by the abdominal method, with 99 deaths (being a death-rate of 72 per cent.), and 276 were cases of vaginal extirpation, with 79 deaths (being a death-rate of 28·6 per cent.).

"Owing to the frightful mortality attending the abdominal operations for the removal of the cancerous uterus," says Dr. J. Reeves Jackson, of Chicago, in an able paper read before the last International Medical Congress, "not less than 72 per cent., and probably considerably more, it has been generally abandoned, except in a comparatively small number of cases in which the vaginal method is inapplicable. The mortality of kolpo-hysterectomy, according to the statistics compiled by Dr. Post, is shown to be 27 per cent. in 341 cases. This is 1 per cent. less than the result of the first 29 operations, collected by Hegar and Kaltenbach, and published six years ago; and 2·4 per cent. greater than the result in 256 cases published by Mundé in September, 1884. The statement has been made that the mortality of the operation is steadily lessening year by year. While this may be true, these tables hardly show the fact. In the case of a few operators improvement has unquestionably occurred, but it is by no means general. In those who survive the operation recurrence takes place on an average in about four months, and death in fourteen months (Sanger), so that the survivors of these 341 operations, numbering 248 persons, would give a total of 289 years, whereas, if uninterfered with, or treated only by palliative measures, they would have lived an aggregate of 413 years—a difference of 125 years. This number added to the 155, the immediate loss by the operation mortality, makes a total of 279 years. The history of surgery surely fails to furnish a parallel to this."

The conclusions with regard to the expediency of the entire extirpation of the cancerous uterus, arrived at by Churchill and Gendrin some forty years ago, are thus, I fear, only too well borne out by the general results of the incomparably better and more scientific methods now employed for the same purpose, and may be here cited. "After," says the former, "a careful examination of the results of the operation, when the uterus is *in situ*, it is really difficult to find adequate reasons in its favour, except the repugnance which every one must feel to give up entirely the hope of affording relief from the most agonising sufferings to which the female sex is exposed—(Churchill's "Diseases of Women," p. 278).

"It is evident," observed M. Gendrin, "that the extirpation of the uterus is one of the gravest and most painful operations in surgery, since it is most fatal. It ought not to be undertaken except with great prudence, nor unless it is probable that the disease is perfectly removable. The signs of this limitation of the disease to the uterus, and of its mobility, are to be acquired by the use of every mode of examining the uterus, but, unfortunately, these means are not always trustworthy. Very able men (MM. Sauter and Roux) have overlooked the extension of the disease to the ovaries and Fallopian tubes), which are often attacked when the body of the womb is affected. We must conclude that in many cases it will be wiser to abstain from the operation.

In considering the respective merits of the early amputation of the cancerous cervix, and the necessarily later complete extirpation of the implicated uterus, the position and surroundings of this organ should, moreover, be borne in view, and especially important in this connection, as Mr. Alban Doran has observed, are the recent researches of Mierzæiosky and Leber on the dense network of lymphatic plexuses surrounding the cervix, and emptying their contents into two large trunks, passing along the broad ligament to the obturator gland, and thus favouring the rapid extension of the cancerous elements into parts almost beyond the possibility of the complete removal of the infected tissues. Under such circumstances there is, I fear, little to be ultimately gained by the more heroic procedure, and our main reliance must rest on the early recognition of the cervical disease, and its prompt treatment by the amputation of the neck of the uterus above the limits of the cancerous infection.

Even in extensive adenoma of the uterus, in which the cavity was filled and enlarged by the malignant villous outgrowth, and the patient exhausted and miserable from the pain, hæmorrhage, and fœtid discharge, I have, without resorting to extirpation, in several instances succeeded not only in relieving those symptoms, but also, as I believe, in materially prolonging the life of the patient, simply by thoroughly scooping out and removing the diseased structures. In two of these cases almost all the uterine

parenchyma was thus so extensively scraped down as to leave only a thin sub-peritoneal shell, and in all of them, after curetting, the raw surfaces were treated with either the perchloride of iron or chloride of zinc. It should be added, that although the disease ultimately returned, and that generally within a year, save in one case in which the patient survived for upwards of two years and then died from other causes, yet the amount of comfort and relief to mental distress, as well as to intense physical suffering, thus afforded, was beyond question.

At the same time there can be little doubt as to the propriety in some urgent and otherwise unrelievable cases of fundal-uterine cancer which are rapidly progressing towards their fatal issue, and are attended with much suffering, of resorting to whatever measure may afford any reasonable possibility of protracting life or of relieving suffering. And under such circumstances extirpation may be expedient, even if it were not capable of effecting in our hands all that has apparently been obtained in Berlin by this operation in the practice of Professor Martin, who states that in 44 extirpations of the uterus by the vaginal method during a period of seven years, the results, *excluding the immediate mortality of the operation*, were 9 relapses within the first year, 4 relapses at later periods, and 31 recovered up to the date of his Report.

Amputation of Cervix as a substitute for Trachelorrhaphy.—In the Transactions of the Washington Meeting of the International Medical Congress, and elsewhere, I have endeavoured to show the advantages of amputation of the cervix uteri over Emmet's operation for the repair of cervical lacerations in certain cases; and as this subject seems to me as deserving of fuller consideration than it has yet received in this country, I now venture to re-submit some of my views thereon, as far as they have been confirmed by more recent experience.

In the last edition of his excellent Treatise on Gynæcology, Dr. Emmet has kindly referred to my views as to the frequency and importance of the obstetric complications arising from cervical laceration, still, neither this point nor the advantages which I believe are obtainable from amputation of the mutilated and

hypertrophied cervix over trachelorrhaphy in certain cases of stellar and extensive bilateral laceration are, I think, sufficiently recognised.

I may, therefore, in this connection, briefly refer to the increasing frequency of cervical lacerations in recent gynæcological practice, and to the causes assignable for this, and point out the parturient and puerperal troubles consequent on these lesions; and, lastly, I shall briefly recapitulate my clinical experience of the treatment of this accident, more particularly in reference to the utility of amputations of the cervix in some cases of extensive bilateral and stellar lacerations. The latter, when met with, in a state of chronic hypertrophic disease, cannot always be remedied by trachelorrhaphy, and may result in life-long misery, even when not eventuating, as already shown is too often the case, in cancer of the cervix uteri.

With regard to the frequency and pathological importance of cervical lacerations generally, my views are entirely in accord with those of Dr. Emmet—namely, that their importance cannot be exaggerated, since at least half the ailments amongst those who have borne children are to be attributed to lacerations of the cervix.

Cervical lacerations are unquestionably now more commonly met with than was the case in my earlier experience. This fact is probably ascribable to the larger proportion of assisted deliveries in recent midwifery practice before the sufficient natural dilatation of the os uteri. As I have before observed, the majority of child-bearing patients who present themselves for uterine examination at my hospital are found to have sustained some degree of cervical laceration, varying from a mere fissuring of the os to the most extensive stellate laceration of the cervix. Even the former must be regarded as a condition of some immediate pathological importance; whilst the latter lesion is of still greater consequence in its secondary results, and its reparative treatment is often followed by the complete subsidence of chronic pelvic complaints, formerly misinterpreted and ascribed to other morbid conditions, or displacements, of the uterus.

The effect on subsequent delivery of cicatrices resulting from cervical lacerations is a matter of practical obstetric interest, these being more frequently the cause of rigidity of the os and tedious labour than is commonly supposed. In my own practice I have seen complete uterine occlusion thus occasioned.

Laceration of the cervix, caused by the premature use of the forceps in the first stage of labour, or from precipitate natural delivery, is, moreover, a subject of considerable importance as one of the causes of flooding. I have before drawn attention to cases of *post-partum* hæmorrhage thus occasioned, of which I have met with many instances. And I may again repeat that there is reason to anticipate that, when the practice recently advocated of applying the forceps before the natural dilatation of the os uteri becomes generally adopted, as seems likely, the next generation of midwifery practitioners will thenceforth have an ampler opportunity of witnessing this accident than was the case in the practice of their, possibly, slower, but certainly safer, predecessors in the obstetric art. For although some authorities hold that the duration of labour, rather than the method of delivery, is the chief factor in the causation of fissurings of the os and cervix, it seems quite evident that no process of natural dilatation, however rapid, can be so liable to occasion rupture of the circular fibres of the os and cervix as its forcible manual extension, in efforts to complete delivery, by version or the forceps. Some years ago I brought cases before the Dublin Obstetrical Society, in which I had been called in consultation, where the cervix had been thus torn through by the abuse of the double-curved midwifery forceps. The same accident I have seen from version, where the hand was forced, rather than insinuated, into the uterine cavity before the full dilatation of the os; and I have been consulted when similar consequences had followed undue manual violence in the removal of a retained placenta. In several cases of this kind I have traced hæmorrhage after delivery to its source in the injured vessels of a lacerated cervix.

The special liability to cervical injuries under these circumstances is self-evident, and their physical evidences are obvious in the hypertrophied, truncated, fissured, or lacerated cervix, with its

irregularly gaping os, semi-concealed by a glairy, or muco-purulent discharge, through which the everted endo-cervical mucous membrane may be seen extruding in too many of our gynæcological patients who have borne children, the result of injudicious or meddlesome midwifery practice.

In a medico-legal aspect, and especially as a proof of previous delivery, cervical laceration is further of importance, since by no other circumstances can the evidences of this injury be produced. On the other hand, the non-existence of such cicatrices or fissures is no proof of non-delivery, as the fissuring may have been so slight as to heal within the puerperal period, leaving no obvious traces of the injury.

Cervical Injuries in Relation to Chronic Uterine Disease.—The connection between laceration of the cervix uteri and many of the chronic disorders of women, formerly exclusively ascribed to sub-acute inflammation of the uterus, and especially to congestion or ulceration of the cervix, has been clearly established. In such cases the lining mucous membrane is forced down through the gaping edges of the rent as soon as the patient rises from the lying-in bed. This cervical ectropium is soon a focus of irritation, spreading upwards and causing endo-cervicitis, the edges of the rent becoming the seat of erosion or chronic follicular ulceration—the hardened cicatricial tissue around the rent, after some time, assuming a distinctive character, and becoming a veritable neoplasm. A more immediate result of bi-lateral cervical ulceration is sub-involution of the uterus, which, as well as pelvic cellulitis and other morbid conditions, may in many cases be ascribed to this cause. In the first instance, the inflammation, extending upwards from a cervical wound to the body and fundus uteri, effectually arrests the natural process of involution. In the latter it spreads along the ligaments, giving rise to para-metritis and salpingitis, or reaches the ovaries, thus causing ovaritis or para-ovaritis.

Cervical Lesions as a Cause of Uterine Displacements.—Lastly, as a cause of uterine flexions and displacements cervical lacerations are of practical interest, for if the resulting cellulitis affects either of the suspensory ligaments so as to cause its thickening and

shortening, the uterus will thereby be thrown out of its normal position, and a constant strain and sense of wearing pelvic pain must be occasioned. These symptoms can only be relieved either by attempting to restore the ligaments to their normal condition by Alexander's operation (in the permanent utility of which I have myself very little faith, as I believe that the shortened ligament must again become stretched if the cause of its lengthening remains uncured), or, and more easily, by taking off the strain of the down-dragging uterine weight by a properly-adjusted pessary. Hence the benefit experienced from mechanical support in many cases of chronic, bearing-down pelvic pain, which is probably as often consequent on cervical laceration as on any primary displacement of the uterus.

Treatment of Cervical Lacerations.—In the majority of instances the direction of the fissure is antero-posterior, and it may extend through both walls of the cervix, or, as is more commonly the case, be limited to its anterior aspect. When thus situated, these fissures, if superficial, often occasion very little trouble, and in many such cases they become healed without any special care during the period of convalescence after delivery. But when from the abuse of instruments, or manual dilatation of the os, to expedite delivery, or for the removal of a retained placenta, or else from the unusual size of the child, or any other circumstance, the cervix is extensively lacerated, either bi-laterally or split into a number of sections by multiple or stellar laceration, then the results of the accident obviously become far more serious, leading to one or other of the pathological conditions just described. Under these circumstances, as a rule, surgical treatment—viz., either trachelorrhaphy or amputation of the cervix—will be necessary. The great and general value of the former, and the procedure by which it may be accomplished, are too familiar to need any reference in this connection.

Advantages of Cervical Amputation in the cases referred to.—My present object is chiefly to call attention to the fact, on which I have elsewhere dwelt—namely, that trachelorrhaphy, however useful in all appropriate instances, is by no means invariably applicable in cases of cervical laceration, occasionally failing even

under apparently favourable circumstances, and that in some instances we may best treat the long train of symptoms consequent on the injuries by the amputation of the lacerated cervix.

This view is not in accordance with Dr. Emmet's opinions. But though none can more fully recognise Dr. Emmet's authority on this subject, which he has made so peculiarly his own, yet I cannot agree with him that amputation of the cervix, except for malignant disease, is necessarily malpractice. The removal of the neck of the uterus for so-called hypertrophy or for abnormal elongation, is also deprecated by Dr. Emmet, who, moreover, is equally opposed to the application of the cautery, or of caustics, to heal a so-called ulceration on surfaces that may possibly be brought into a healthy condition and united by his operation. "If," in Dr. Emmet's words, "this so-called ulceration, or this elongated cervix, should prove to be merely a laceration, the sides of which can be brought together and united so that the integrity of the parts will be as perfect as if the accident had never occurred, then to resort to amputation is malpractice." This proposition is self-evident. But in that little *if* lies the entire question; and highly as I value trachelorrhaphy, and successful as I have found it in many appropriate cases, I must repeat that in certain cases of extensive stellate lacerations of long standing, with considerable loss of substance, and accompanied with chronic inflammatory conditions of the adjacent structures, or cellulitis, as well as with great hypertrophy or hyperplasia of the injured cervix, caused by inflammatory exudations, trachelorrhaphy is inapplicable, and such cases may, I believe, be better treated by the amputation of the mutilated and diseased cervix. Moreover, this operation, according to my experience, is fully justifiable in some cases of non-traumatic hyperplasia, and chronic parenchymatous cervicitis, more especially, as I have already pointed out, in patients who are hereditarily predisposed to malignant disease, as well as in some instances of cancer of the cervix, either as the most certain prophylactic of uterine cancer, or else as a possibly curative measure in the early period of the disease, or if for nothing else than merely to relieve suffering, if not to prolong life in its latter stage. Finally, although

in the majority of cases of laceration in which I have removed the cervix, the patients have since remained sterile, still, on the other hand, I do not therefore question the fact averred by other gynæcologists that this operation may possibly be called for in some cases of sterility, resulting from the mechanical obstacle to impregnation offered by a greatly hypertrophied, elongated, and conical cervix uteri.

Within the last few years I have, for the before-stated reasons, amputated the cervix in a considerable number of cases, and so far, at least, I have had no experience of the disastrous consequences which some writers insist on ascribing to this operation.

In conclusion, I have only to express the hope that the foregoing observations may conduce to a more frequent and earlier resort to amputation of the cervix, in the prevention and treatment of uterine cancer, as well as a substitute in appropriate cases for cervical trachelorrhaphy. And although my views on the latter point have been elsewhere expressed, yet, inasmuch as the subject discussed is one, as I believe, very unduly neglected in this country, I have included some of the opinions I have long maintained on this subject, together with others which may be found novel, in the hope of inducing the thorough discussion of two closely connected questions, which appear to me of practical importance, and well deserving the consideration of the Academy.

A CASE OF LAPAROTOMY IN ECTOPIC GESTATION.

By WILLIAM THORNLEY STOKER,

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[Read in the Section of Obstetrics, March 8, 1889.]

On the night of Saturday, January 19th, 1889, a married woman, named A. B., aged twenty-four years, was sent into the Richmond Hospital as a case of intestinal obstruction, but as I had no professional account of her illness, I was dependent on the patient and her friends for her surgical history, which added to the difficulties of diagnosis. Her previous history, as far as it could be elicited, was as follows:—She had been married for four years, and six and a half months after marriage had given birth to a dead child. She did not again conceive until the pregnancy which caused her death, but she constantly suffered from pain in the left inguinal region, from which she sought relief at several Dublin hospitals. She menstruated regularly, if painfully, until November, 1888, missing the catamenial period in that month and in December. She thought, therefore, she was pregnant, but menstruated again on January 5th—the discharge being, as is now evident, one of those imperfect menstruations seen in ectopic cases. On December 24th she was attacked by acute pain in the left inguinal region, for which she sought medical relief.

On Saturday, January 12th, whilst walking in the street, she was again seized by this pain, but of even a more acute character. She appears to have become partially collapsed, for she had to remain three hours in a neighbour's house where she took refuge; she then returned home, and was placed under medical treatment. During the week which intervened between the 12th and 19th of

January she continued to suffer the most acute pain, was without sleep, vomited every time she took food, and had no action of her bowels. Her treatment, I believe, consisted of a daily succession of purgatives and enemata.

When she came under my observation she was in a most exhausted and distressing condition—crying aloud from acute abdominal pain, having an anxious worn-out expression, a rapid pulse of poor quality, and a temperature of 98°. Her skin was clammy, her tongue brown, and she vomited frequently, but not fæcal matter. The abdomen was tumid and tympanitic, but not to an excessive degree; the regions of the ascending and descending colon were dull on percussion, and a small circumscribed swelling could be felt in the left inguinal region, immediately above Poupart's ligament. The rectum was empty, but collapsed from pressure; the uterus pushed down into the pelvis, and fixed; and a marked tumefaction could be felt in the recto-vaginal pouch.

The case was evidently both uncertain as to its diagnosis, and urgent in its condition. It demanded a laparotomy for the purpose of ascertaining its nature, and, if possible, procuring its relief. But, having regard to the length of time—over seven days—which had elapsed since the commencement of the attack, and to the fact that the woman, although much reduced, was not in a condition to suffer from an added delay of a few hours, during which she might be prepared for an operation by inducing some sleep, I decided to postpone the laparotomy until morning, so as to have the advantage of daylight. I was further induced to this delay because, although the bowels had not acted for a week, the case did not in its general aspect seem like one of primary obstruction. The history of previous menstrual trouble, of a sudden attack when in ordinary health, with the immediate and continuous severe pain and early depression, pointed away from the usual conditions found in simple intestinal stoppage. The absence, however, of any accurate history prevented a positive diagnosis being come to.

I attempted to pass a long tube; as usual, it did not enter for more than eight or nine inches, and the warm water which I introduced returned at once. Pending the adoption of operative

measures, stimulants were administered, and opium given in quantities sufficient to relieve pain.

During the night the woman slept a little; was comparatively free from pain, owing to the opium, but vomited frequently, the ejected matter becoming fæcal in character.

In the morning her condition was such that my colleagues, Messrs. Thomson and Corley, agreed with me that laparotomy should be undertaken at once. She was, therefore, put under ether, the abdomen opened by a median incision, and the omentum found to be so firmly adherent to the anterior pelvic wall that it could not be detached by any allowable degree of force. I was obliged to include nearly its entire width in a series of transverse ligatures, and to divide it above its lower connection. The peritoneum was then discovered, as had been anticipated, to be extensively inflamed, and its pelvic and lower abdominal sac to be the seat of an hæmatocele, and filled with quantities of black, fluid blood and masses of clots. The intestines in the corresponding parts of the cavity were densely matted together by adhesions, and in the upper region of the abdomen were deeply congested.

Search discovered a tumour in the left broad ligament about the size of a small hen-egg; it was evidently the seat of an ectopic gestation, and the source of the hæmorrhage. It was tubal and free from the ovary and uterus.

The great intestine was comparatively empty, the small intestine much distended, and the obstruction due to pressure by the clots on the small bowel, where it rises from the pelvis to enter the colon, and on the pelvic portion of the greater bowel itself.

The bladder was contracted and so bound down by adhesions that it was incapable of distension, and felt like the unimpregnated womb of a woman who had borne children.

The tubal growth was included between two silk ligatures passed through the broad ligament, so as to include a V-shaped surface, and was then removed. The clots were cleared out of the peritoneum by manipulation and washing, and intestines, as far as possible, freed from adhesions. The cavity was then sponged out,

a large drainage tube introduced, the abdominal wound closed, and dressings of sublimated wood-wool applied.

The operation was difficult and protracted, and the condition of the patient and the parts such that but little hope of her recovery was entertained. She remained comparatively free from pain, became gradually weaker, and died in about eight hours after the abdominal wound had been closed, worn out by previous sufferings, and the shock of the necessarily prolonged laparotomy.

The sectio cadaveris fully endorsed the description I have given of the parts as discovered during the operation. The oval mass, which was removed, had been situated in the broad ligament, and involved the portion of the left Fallopian tube near its fimbriated end, leaving over an inch of tube next the uterus free from any tumefaction. The ovary as well as the uterus was free from engagement, and the gestation, therefore, tubal in the most strict sense of the word. The parts after removal were submitted to Professor Fraser, who has kindly furnished me with the following notes of their condition, which correspond to my own observations of them :—

"The tumour consists of a portion of the left Fallopian tube, the size of a hen's egg, more or less ovoid in shape. At the two poles the openings of the tube (cut) could be easily recognised. On making an incision along what had been the free border of the tube, the contents of the dilated portion were laid bare. The walls of this portion were quite free from the contents, except at one portion of the circumference next the broad ligament attachment. The contents were found to be the placental portion of the embryo, distended with coagulated blood, attached to that part of the circumference of the dilated tube above noted. They were solid to the touch, and I could discover, on rough examination, no embryo or free membranes. On section, however, the naked eye characters of the placenta could be easily recognised, the sinuses being full of coagulated blood.

"The uterus was enlarged, the walls being much thickened and its cavity empty; the cervical canal, however, was closed by thick, stringy mucus. The left ovary on long section was healthy;

Gräffian follicles and remains of old corpora lutea could be seen. That portion of the Fallopian tube attached to the uterus, although enlarged, was also apparently healthy, On the right side the ovary was found to be healthy, although it was imbedded in the posterior wall of a cyst about the size of a hen's egg. The Fallopian tube could also be traced along the anterior wall of this cyst, then over its posterior surface to the ovary; its termination was marked by the presence of a small cyst with a long peduncle. The outer half of this tube was not normal in appearance. The large cyst, on being opened, had several secondary ones in its interior, and was probably parovarial in origin."

The extravasation of blood was entirely intra-peritoneal, with the exception of the clots contained in the distended tube removed during operation. No other blood had passed between the folds of the broad ligament, or into any extra-peritoneal situation whatever.

The theory of the case is probably this:—The hæmorrhage into the tube took place when she suffered the first attack of acute pain on Christmas Eve. The bleeding into the peritoneum was subsequent and secondary to this, and began at the date of her second and fatal seizure on January 12th.

When the imperfect clinical history of this case is read with the light of *post-mortem* evidence, its story is, in most respects, the usual one, but in a few particulars peculiar or uncommon.

It is usual, in its incidents of a pregnancy followed by a long period of sterility; and of its history of disordered menstruation and pain—probably tubal in origin, and due to some cause such as desquamative salpingitis. It is usual as to the period at which the fatal hæmorrhage took place, for as she had her last normal menstruation about October 5th, and her fatal attack of bleeding on January 12th, her gestation was probably advanced at the latter date to about its twelfth week, and was certainly under its fourteenth. Again, the ordinary rule in these cases is the appearance of an irregular menstruation when one or more periods have been skipped. The detection of a tumour in the inguinal region, due to the hæmatocele in the broad ligament, is another matter of previous observation, and one which has before now helped to a

diagnosis. All these are points ordinarily lending themselves to the elucidation of this accident; and as regards the previous tubal trouble—which appears to have existed here—it is a condition so usual that it has generated and supported the theory that disease of the tube is responsible for the ectopic gestation, by offering a mechanical obstruction to the passage of the ovum. It is doubtful if a tubal pregnancy ever occurs save in a person who has suffered from salpingitis, or some condition capable of obstructing the ovum. This case is an example of the tubal or more common of the forms of ectopic gestation.

With these facts before us, it is evident that the case was one which, if it came under observation at the commencement of the hæmorrhage, might have been recognised and saved by the operation which, as it was, proved too late. The results which Mr. Lawson Tait has obtained in these cases is such as to force the question of operation on surgeons.

It is unfortunate that as yet we have no means of making more than a guess at the existence of an ectopic pregnancy until the accident of a hæmorrhage tells the story with sufficient emphasis. Once that has occurred the distinction has to be made between extra- and intra-peritoneal bleeding. If it be outside the peritoneal sac, the interference of the surgeon is less urgently demanded, and his procedure less definite. But if the bleeding be intra-peritoneal, laparotomy, with removal of the embryo and the parts containing it, and a cleansing of the peritoneum, is the only logical course to follow. Mr. Tait has recorded 41 cases of intra-peritoneal hæmorrhage due to ruptured tubal gestation in which he has operated, with but two deaths—a result which, having regard to the awful fatality of this accident when the abdomen is not opened, speaks trumpet-tongued as to the propriety of active surgical treatment.

I have stated the usual opinion that, whilst delay may be made in dealing with an extra-peritoneal hæmatocele, immediate treatment is required where the blood is within the serous sac itself. Among other reasons for this difference is the condition of the extravasated blood. When it lies in extra-peritoneal tissues it has

the ordinary tendency to coagulate; on the other hand, when within the peritoneum its unvarying inclination is to remain fluid or imperfectly coagulated, and thus to promote the hæmorrhagic tendency. This is probably due, not only to the nature of the surface with which it lies in contact, but also to the admixture with the fluid excreted from that surface. The case in point affords a good example of these conditions, for while the clots in the tubal sac were so well formed and laminated as to resemble the contents of an aneurysm, the blood in the peritoneum was partly fluid, none of it thoroughly coagulated, and all of it black and tarry in appearance.

In a few particulars this case is a somewhat exceptional one. Although both intra- and extra-peritoneal hæmorrhages are common as a result of ectopic gestation, Mr. Tait has been able in his lectures to mention only two examples such as this, in which a primary extra-peritoneal bleeding was followed by a secondary one into the sac of the peritoneum itself. One of these is the case of Goupil, in 1855, and the other that of Duverney, in 1712. From a careful examination of the specimens in my case, I am of opinion that the secondary hæmorrhage was not due to a rupture of the primary sac, but to a penetration from the tube into the peritoneum of the radicles of the placenta. Existence and growth of the placenta was possible, even if the rest of the embryo had been devitalised by the first intra-tubal hæmorrhage. No point of rupture could be discovered on the peritoneal covering of the tube, or in the tube itself, but on the peritoneum covering the broad ligament attachment of the tube, and corresponding to the situation of the placenta, a small circular area, about 1 cm. in diameter, of a deep red colour and great vascularity, could be seen. From this placental penetration I believe the secondary hæmorrhage to have come. There was no blood between the layers of the broad ligament, except those well-formed clots contained in the sac formed by the dilated Fallopian tube.

The second exceptional feature to which I would point is the contradiction offered to the statement of Mr. Lawson Tait, who, speaking of blocking of the rectum, caused by constriction due to

blood travelling between the folds of the broad ligament, says of the rectal obstruction—"This is one of the signs of broad ligament hæmatocele which has not yet been noted by any writer with whose works I am acquainted, and it is of great importance. It does not—indeed it cannot—occur in intra-peritoneal effusion." That this statement requires modification is shown by the conditions existing here, where intestinal obstruction was present from the very beginning, and in which the bowel was stopped by a pressure purely intra-peritoneal.

I have felt myself justified in dealing with the details of this case, not only because it possesses one or two features of exceptional interest, but because the treatment of the accident in question by laparotomy is a comparatively new one, and one about the propriety of which I hope most surgeons will agree. That most of its features are typical, and therefore ordinary, would be a reason for remaining silent if the literature of the condition was extensive, or its diagnosis and treatment well established. But where this is not so, the case has not only some value in itself, but its details may serve as a guide by calling attention to a terrible surgical emergency, the treatment of which has lain in abeyance too long.

ON THE TREATMENT OF PROLAPSE OF THE UTERUS BY MASSAGE AND PELVIC GYMNASTICS.

By ALFRED J. SMITH, M.B., M.Ch., M.A.O., R.U.I.;
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[Read in the Section of Obstetrics, April 12, 1889.]

As a supplement to the President's valuable contribution on Prolapse of the Uterus, I venture to bring forward a system of treatment which I have been employing in cases of prolapse for some time past at the Rotunda Hospital. I followed as accurately as possible the method of massage and pelvic gymnastics taught by Major Thure Brandt, of Stockholm, and lately introduced to the notice of the profession by Dr. Paul Profanter in his monograph, "The Manual Treatment of Prolapse of the Uterus." I am indebted to Dr. Macan, Master of the Rotunda, for first bringing under my notice this extremely interesting pamphlet. The *technique* of this special treatment consists of—(1), in the lifting of the uterus; (2), in massage of the uterus and its ligaments; (3), in forced separation and forced closure of the knees; and (4), in tapotement of the lumbar and sacral vertebræ.

(1.) *Lifting of the Uterus.*—The patient lies in the lithotomy position on a low couch, with her clothes loose about her hips and corset open; the bladder must be empty and abdominal walls relaxed; an assistant sitting to the left of the patient replaces the uterus bimanually, and maps out with his external hand the position of the fundus. The operator, facing the patient, rests his left knee on the couch, his right foot on the floor, then laying his hands flat on the abdomen, with finger tips turned towards and almost touching the pubis, he presses deeply into the pelvis and grasps the uterus—which organ is prevented from being pushed into the

hollow of the sacrum by the assistant's finger in the vagina. Now leaning over the patient, the assistant at the same time removes his external hand, the operator raises the uterus gently and slowly in the axis of the pelvis as much over its normal level as it had formerly been prolapsed. When the uterus has been elevated sufficiently to put on a stretch the relaxed parts, it is allowed to slip gently from between the hands, and sink slowly in the abdomen. The assistant's duty is now to receive the uterus as it descends on his finger—which has remained in the vagina—and keep it in a condition of anteflexion. This lifting of the uterus must be repeated three or four times in a daily sitting, a few minutes' interval being allowed between each, during which the assistant gently massages the uterus. The elevation must be done gently, hence it is advisable and necessary to constantly watch the patient's face for the slightest expression of pain. By elevation the vagina, the uterine ligaments, the peritoneum, with its unstriped muscular fibre, the nerves and blood vessels are all stimulated; the mechanical irritation causes not only a retraction of the involuntary muscle fibre, starting from the point of irritation, but induces a long continuous contraction. When daily practised this artificial exercise has the effect of natural movement, of developing relaxed and weakened structures, restoring them in a marked degree to their original healthy condition; and further, any bands or adhesions which may have formed between the uterus and neighbouring organs are either relaxed or wholly separated.

(2.) *Massage of the uterus and its ligaments* is performed during the pause of some minutes' duration, which follows each lifting of the uterus. It consists of stroking movements applied to the fundus of the uterus in the direction of the internal os, in order to stimulate contractions and empty the numerous distended veins. Massage is generally performed by the assistant's external hand, the finger in the vagina being used only as a support to elevate the part against the abdominal walls and fix it there.

(3.) *Forced Separation and forced Closure of the Knees.*—The patient being still in the lithotomy position, should close her knees and thighs, and elevate the sacrum from the couch, supporting the

body weight on the elbows and feet only; then the operator seizes the knees and forcibly abducts, while counter-resistance is offered by the patient. The contrary movement is now practised—the patient lies with knees widely separated, and while she forcibly closes her thighs the operator performs forced abduction. This combined action is generally performed from three to six times during each sitting; it specially aims at the development of the muscles of the pelvic floor—more particularly the levator ani—and that it powerfully stimulates this muscle any person can demonstrate for himself.

(4.) *In tapotement* the lumbar and sacral vertebræ are percussed several times with the clenched hand. Thure Brandt considers it acts by stimulating the nerves of the true pelvis, thus aiding in the restoration of the uterine supports. It is an exercise which did not commend itself to me, consequently I did not practise it.

The duration of these four exercises—which are performed at one sitting—is from fifteen to twenty minutes, and should be repeated daily for a period of from two to eight weeks. The sitting having concluded, the patient turns on her face, and remains in that position for about ten minutes. The importance of regular attendance during the period above specified must be impressed on the patient, who, perhaps, feeling her condition improved, is apt to become irregular in her attendance. A caution must also be given against going up flights of stairs or lifting heavy weights. I now ask the indulgence of the Section while I read notes of six cases in which I practised this line of treatment, and I may here take this opportunity of returning thanks to Dr. Bagot, my colleague, for his skilled assistance:—

Case I.—Mrs. C., aged fifty-four, five children; came to the Rotunda Dispensay, Jan. 16th, 1888, suffering from prolapse of the uterus.

History.—Her health was good up to a year ago, when she began to suffer from pains in the small of the back, and a great sinking sensation in the pelvis. In a short time this was followed by protrusion of the uterus outside the vulva; she never wore instru-

ments. Her youngest child is fifteen, and she is twelve years past the change of life.

Examination.—The anterior and posterior vaginal walls are quite dry and parchment-like; the cervix is eroded and weeping; the uterus is 9 cm. long, lying outside the vulva, easily reducible; ovary lying in posterior *cul-de-sac.*

Treatment.—Uterine elevation and massage; pelvic gymnastics as described above.

Jan. 17th, slight prolapse of vaginal walls on coughing; Jan. 18th, 19th, do. Jan. 21st, uterus retroverted to second bone of the sacrum; no prolapse on coughing, has no pain in sacral region, feels comfortable and firm, with no tendency to prolapse, although suffering from a bad cough. Jan. 23rd, uterus again prolapsed. Jan. 24th, uterus retroverted to first sacral bone; no prolapse on coughing. Jan. 26th, do.; Jan. 28th, do. Feb. 2nd, uterus has not come down since last day; feels quite firm, and in every way better, except for slight pain in sacral region. Uterus retroverted to first sacral bone. Exhibited patient to Dr. Macan in Surgery; he got her to cough without any prolapse even of vaginal walls. Feb. 4th, 8th, do.; Feb. 12th, do. August 2nd, patient stayed away contrary to my wishes; I exhibited her to class. She stated her womb had not come down since, and she felt quite strong.

Case II.—Esther I., aged thirty-two, one child, a year and ten months old; came to the Rotunda Hospital, suffering from prolapse of the uterus, on February 10th, 1888.

History.—She enjoyed good health until two months after her confinement, when she noticed "a lump coming down in her passage." She is a small, delicate woman, and attributes her condition to carrying about a heavy baby. She brought in her hand a No. 8 Hodge pessary, which was useless to her.

Examination.—Cervix hypertrophic, and protruding through vulva; no vaginal folds or patulous erosion. On coughing anterior and posterior vaginal walls prolapse; uterus length, 9 cm.; perinæum lacerated slightly.

Treatment, as above.

Feb. 12th, feels firmer. Feb. 13th, uterus remained up until ten a.m. this morning. Feb. 14th, 15th, 16th, uterus retroflexed. Feb. 17th, do. Feb. 18th, uterus does not prolapse on coughing. Feb. 19th, uterus prolapsed. Feb. 20th, do. Feb. 24th, 26th, 27th, uterus prolapsed. No. 8 Hodge introduced.

This patient was delivered in Rotunda during last November of a full term child.

Case III.—Catherine C., aged forty-five, two children, was admitted to No. 10 Ward, Rotunda Hospital, June 22nd, 1888. She complained of her womb coming down, and pain in passing water.

History.—Has had prolapse of the uterus for four or five years, especially for the past twelve months; she suffered great inconvenience while working or sitting. Uterus could not be kept up by a No. 8 Hodge pessary—dysuria.

Examination.—Vaginal mucous membrane turned into skin; ulceration around os; complete prolapse on coughing; large cystocele; uterus retroflexed, 10 cm. long.

Treatment, as above.

June 23rd, lying in bed. Uterus did not come down. June 24th, 25th, 26th, do. June 27th, up for three hours, no prolapse. June 28th, up for one hour, tendency to vaginal prolapse; from 30th, prolapse of vaginal walls; uterus prolapsed on coughing. July 2nd, menstruating, no internal manipulation, gymnastics practised. July 4th, not prolapsed on coughing; 5th, 6th, 7th, do.; 8th, 9th, 11th, uterus prolapsed on coughing, No. 8 Hodge introduced. Dec. 17th, 1888, uterus in position, pessary can easily be retained, gives great relief; washed and re-introduced.

Case IV.—Bridget R., aged fifty-eight, five children; came to Rotunda Dispensary, June 28th, 1888, suffering from prolapse of the uterus.

History.—Her menstruation began when sixteen years old; she had five children, the youngest of whom is twenty years. She had no trouble with any of her confinements; has been suffering for the past three years from her womb coming down, particularly for the past twelve months. The uterus returns when the patient sits, but falls down immediately on walking or making any exertion; she suffers from a red discharge; has a bearing-down sensation in pelvis, and pain in sacral region.

Examination.—Vaginal walls œdematous and leathery; uterus $10\frac{1}{2}$ cm.; perinæum slightly lacerated; uterus projected on coughing completely outside vulva; slight cystocele.

Treatment, as above.

June 29th, uterus remained up one hour; came down more than

usual after she walked home. July 2nd, uterus remained up since last day; prolapsed on coughing; length, 8 cm. July 3rd, no prolapse on coughing; uterus antiflexed; 4th, 6th, 9th, 10th, uterus still normal, feels quite strong; 11th, showed to class; 13th, uterus still in position, no prolapse on coughing; 14th, do.; 16th, do. Told patient to stay away for a couple of days, but to return at once if her womb came down. She has not since returned.

Case V.—Catherine M., aged sixty, three children; came to Dispensary, June 28th, 1888. She complained of her womb coming down on the slighest effort.

History.—She is ten years past the change of life. From the birth of her youngest child, fourteen years old, she noticed at every effort her uterus came outside the vulva. Has been treated in the Dispensary with Mayer's ring, No. 8, which gave her relief at first, but latterly came out when making any exertion.

Examination.—Vaginal mucous membrane dry and leathery; cervix quite small, flush with vaginal walls on coughing; the uterus and vagina are projected balloon-like from the vulva; uterus length, 4 cm.; it is easily reducible, quite small, and atrophied.

Treatment, as above.

June 29th, on reaching her home went to bed; uterus remained up until this morning. Complains of sacral pains. June 30th, uterus prolapsed. July 2nd, uterus prolapsed; sacral pain. July 3rd, complains of great sacral pain—dysuria; 4th, 5th, 6th, 7th, do.; uterus and vagina prolapsed on coughing. July 10th, do.; No. 8 Mayer's ring introduced. Dec. 17th, no prolapse of uterus or vaginal walls on coughing; pessary gave greatest relief.

Case VI.—Mrs. S., aged fifty, two children; came to my study, Nov. 17th, 1888, suffering from falling down of the uterus.

History.—Her last confinement was twenty years ago, since which time she has suffered from sacral pain and prolapse of the uterus, which for the past year caused her great annoyance and discomfort.

Examination.—Vaginal walls like skin, quite dry; uterus, 10 cm., partially prolapsed, does not completely come outside the vulva on coughing; slight cystocele.

Treatment, as above.

Nov. 21st, no improvement. Nov. 23rd, do. Nov. 26th, uterus remained up since last day; came down on coughing. Nov. 29th,

uterus normal; feels quite strong and firm. Jan. 7th, 1888, felt quite well up to the past two days, when the uterus again became prolapsed. As patient lives at Dundrum, she could not attend regularly. Pessary, $9\frac{1}{2}$ Hodge, introduced.

To sum up, Case I. shows a distinct improvement of six months' duration; from Case II. no conclusion can he drawn, as she became pregnant. Case IV. was under observation for sixteen days, during which time the uterus remained in its normal position. Cases III. and V. were at first exceedingly disappointing, inasmuch as it was necessary to fall back on the pessary. But when we consider how useless this instrument was before the course of treatment, and the comfort and benefit its use gave to the patients afterwards, I feel justified in ascribing this marked improvement to the massage and pelvic gymnastics. Case VI. was most promising, but the rregularity of the attendance, owing to the distance from hospital, prevented me from continuing the treatment.

These results are not so brilliant as those obtained and recorded by Paul Profanter,[a] Thüre Brandt,[b] Professor Von Preuschen,[c] Schauta,[d] and others, and might even be considered by some inconclusive. Yet my reason for bringing this record of cases before you is not so much on account of the brilliancy of the results obtained as that I believe this special treatment is one capable not only of effecting permanent cures, but certain to prove a powerful auxiliary to the pessary in the treatment of prolapse, and, further, promises to be of great use in many cases of retroflexion or version—an essential condition of which is a descent of the uterus; therefore, any such method cannot but claim our earnest consideration.

[a] Die Manuelle Behandlung des Prolapsus Uteri.
[b] Heilgymnastische Behandlung weiblicher Unterleibskrankheiten.
[c] Centralblatt für Gynækologie. No. 13. 1888.

ON THE DIAGNOSIS AND TREATMENT OF SMALL SUBMUCOUS FIBRO-MYOMATA OF THE UTERUS.

By W. J. SMYLY, M.D.,
Obstetric Physician to the City of Dublin Hospital.

[Read in the Section of Obstetrics, May 24, 1889.]

There is perhaps no subject in gynæcology upon which authorities differ so widely, as upon the treatment of uterine fibroids. In the present communication, however, I purpose confining myself to a very limited class of these tumours—those, namely, which protrude into the uterine cavity, and are not larger than a hen's egg. I limit myself to tumours of this size because we are generally agreed that they should be removed per vaginam, whilst in the case of larger growths there is not the same unanimity, and I for my part agree with those who hold that, excepting only polypi which have been partially or entirely expelled through the os, they should not be removed in this way. The diagnosis of these tumours, until recently, presented no difficulties. So long as the rule obtained *in every case of serious uterine hæmorrhage not connected with existing pregnancy to dilate the uterus and explore the cavity with the finger*, they could scarcely escape detection. But since the introduction of the curette has rendered this painful and somewhat dangerous preliminary in the great majority of cases unnecessary, it is quite possible that they might do so, and when some years ago I brought the subject of the diagnosis and treatment of the diseases of the uterine mucous membrane without previous dilatation of the cervix, before this Section of the Royal Academy of Medicine, it was urged as one of the strongest objections to it, that new growths of this kind might be overlooked. Not being able at that time to answer this objection from my own personal experience, I quoted the words of Prof.

Fritsch, who said "that it is impossible to discover and remove small polypi by means of the curette without actually feeling them, will only be believed by those who have no experience in this method," (Die Lageveränderungen und Eztundungen der Gebärmutter, p. 1017). Now if it be possible to detect small polypi by this means it must obviously be easier to detect larger tumours, and I can now, from personal experience, corroborate the above statement. In some cases, however, the necessity for dilatation is suggested by the condition of the cervix, or the history of the case. The two following cases will serve as illustrations of this :—

Case 1.—Mrs. H., the mother of eight children, the youngest $2\frac{1}{2}$ years of age, told me that for five years previously to my seeing her, excepting only when she was pregnant, she had suffered from profuse menstruation, which had become much worse lately, lasting a fortnight very profusely and with expulsion of large clots. She was very anæmic, suffered from sleeplessness, and was generally in very bad health. Physical examination showed the uterus enlarged, the sound passing to a depth of $3\frac{1}{2}$ inches, and its withdrawal followed by some hæmorrhage.

On the 17th of May last year, I had the patient etherised with the intention of curetting the uterus, but upon drawing down the organ I found the os so patulous, that No. 16 Hegar passed into the cavity with ease. This I considered a sufficient indication for the introduction of the finger, and I then found a small sessile fibroid about the size of a walnut protruding from the posterior wall of the uterus about half an inch above the internal os. Unfortunately, I had not the necessary instruments at hand, and was obliged to postpone its removal for a couple of days. I then dilated up to 22 Hegar, and got it away without difficulty with Schultze's spoon forceps.

The second case came to me in 1885, complaining of violent hæmorrhages. She was very anæmic, and thought that she was going to die. She had been for a long time under the care of a physician who never examined her, but on the contrary cautioned her against permitting anything of the kind. I found a fibrous polypus about the size of a hen's egg protruding through the os into the vagina, which I easily removed with an ecraseur. After this the hæmorrhages decreased and her general health improved very much, but after a time they returned as violently as before, and on

the 22nd of January last, she was admitted to the City of Dublin Hospital. The uterus was now found to be very much enlarged, the sound passing to a depth of four inches.

The history of this case would have led me to explore the uterus, but besides this the sound gave me the impression that there was something inside. I dilated the cervix with iodoform gauze. The uterus was packed with the gauze on two successive days, and on the third morning I found sufficient room to admit my finger and the spoon forceps. I first came upon a sessile fibroid about the size of a pullet's egg springing from the right wall of the uterus just above the os internum. Having removed this, I found a small polypus about the size of a marble attached to the left side of the uterus close to the opening of the fallopian tube. This was seized with the same forceps and twisted off. Two other tumours were found, one in the left wall, and the other in the fundus, but so deeply embedded that I did not think it advisable to interfere with them. The patient made a good recovery, and has improved very much in health.

In the two following cases the indication for dilatation was the detection of the new growth by means of the curette.

Case I.—J., aged 36, unmarried. Had suffered for some months from menorrhagia and dysmenorrhœa. The uterus was normal in shape and position but somewhat enlarged. The sound passed to a depth of three inches, but I could not by means of it detect anything else abnormal.

On the 16th of last November I curetted the uterus, and distinctly felt that the instrument caught in something in the neighbourhood of the fundus which I could not bring away. I accordingly plugged the organ with iodoform gauze and two days later completed the dilatation with Hegar's dilators. Finding a small sessile tumour growing from the fundus I removed it with the spoon forceps.

Case II.—Mrs. T., aged 25, married fourteen months, was admitted into the City of Dublin Hospital in January of this year. She had no child, but had aborted five weeks previously. Since then had constant metrorrhagia with pain in the back and hypogastrium. On examination the uterus was found to be enlarged. Believing that some portion of the ovum or decidua was retained in utero I endeavoured to remove it with the curette, but I soon became conscious that there was something in the womb which I could not

get away by this means. Still supposing this to be part of the ovum I dilated with iodoform gauze, and afterwards with Hegar's dilators. Upon introducing my finger I was surprised to find a small fibroid attached to the fundus which I removed as in the other cases. I also found that during the process of dilatation the cervix had been torn on the right side, and required two stitches to restore it, and that a small fissure could be felt on either side passing upwards through the internal os.

Now these cases suggest some points which are open to discussion, and which are, to myself at any rate, of much interest—and, firstly, what are the indications for dilatation of the cervix and exploration of the uterus? I believe that this is necessary only in order to remove tumours, polypi, and portions of the ovum, but that hæmorrhage alone is not an indication. Secondly, what is the best and safest method to adopt? Hegar's dilators are good, but they always cause more or less bruising and tearing of the cervical tissues, so that where rapid dilatation is necessary some of the best operators prefer the knife, a clean wound being in their opinion better than a lacerated one. For my own part I prefer a more gradual method, unless the cervix be already somewhat patulous, or at least easily dilatable. Lawson Tait's are theoretically the best dilators, but require too constant supervision, and, for the present at least, I prefer iodoform gauze to any other method. With this the cervix softens and dilates as in abortion, being retracted over the plug, and not expanded by excentric pressure, and the method is therefore much less painful than the use of sea-tangle or other expanding tents. The plug is also quite aseptic, and may be left *in situ*, if necessary, for three or four days, and is therefore especially adapted to the needs of the general practitioner.

Lastly, for the removal of small tumours no instrument is so safe as Schultze's spoon forceps. With them diseased tissue alone is attacked, and with ordinary care no harm can be done. Enucleation is, on the other hand, much more difficult, and dangerous, and injury and even perforation of the uterus are not remote possibilities. The *écraseur* is not adapted to these cases and, excepting only polypi with narrow pedicles, is a very unmanageable instrument.

SECTION OF PATHOLOGY.

CASE OF THORACIC ANEURYSM; CURED POPLITEAL ANEURYSM.

By WALTER G. SMITH, M.D.;
Physician to Sir Patrick Dun's Hospital.

[Read in the Section of Pathology, November 2, 1888.]

James F., aged fifty-four years, was admitted to Sir P. Dun's Hospital, 18th May, 1888.[a] His family history was good, and he himself, although in the habit of drinking freely, was a healthy man, and accustomed to hard work. His occupation as a porter involved the frequent lifting of heavy weights.

Thirty-four years ago he had contracted a venereal sore, the scar of which remains at the base of the prepuce, and traces are left of a suppurating bubo in the left groin. Some seven or eight years ago he was admitted into Jervis-street Hospital under Dr. Cranny and was submitted to treatment by "pressure" for a tumour which existed in the left popliteal space.

Six months ago he suffered from a severe cough, which was ascribed to a wetting, and since that time he did not feel so well as formerly, complaining of pain in the left side. Still he was fairly well up to Thursday, April 10th, 1888, upon which day while attending to his business he became faint and greatly distressed in his breathing, and was sent as an urgent case into Jervis-street Hospital, where he remained for five weeks.

When he came under observation in Sir P. Dun's Hospital his symptoms were chiefly these:—He was unable to go about owing to shortness of breath, and he suffered from pain over the left side

[a] Clinical notes by Mr. Molloy.

and in the left shoulder. This pain sometimes shot down the arm to the elbow, and he asserted that the left arm was not so strong as it used to be.

Upon inspection, there was observed a bulging of the intercostal spaces on the left side involving the three upper ribs, and a slight projection forwards of the upper end of the sternum. Pulsation was present over the whole of this area, synchronous with the cardiac impulse. The apex beat was displaced downwards between the sixth and seventh ribs.

Percussion revealed an extensive area of dulness. Commencing at the left sterno-clavicular articulation, it curved to the right across the sternum to the edge of the right second rib; thence to the third left costo-sternal articulation, descending straight to fifth rib. Externally the dull area was bounded by a line stretching from middle of clavicle to level of seventh rib, $1\frac{1}{2}$ inches distant from the sternum.

The respirations were rapid—25 per minute. The percussion tones over the back of each lung were normal. Tenderness upon pressure existed over the fourth and tenth dorsal spines. Upon auscultation the breath-sounds were absent over the dull area described above. Some bronchial râles were heard over the back of both lungs.

No cardiac murmur was detected. The second sound had a booming character. The cardiac sounds were plainly audible over the dull area, but no independent nor accompanying murmur could be made out. The pulse on the left side could with difficulty be felt in the axillary, brachial, or radial arteries, and it was perceptibly delayed.

During the whole of his illness the patient persistently complained of sharp, shooting pain down the neck, across the upper part of the chest, down the arm, and in the back. He was sleepless, and the extremities cold.

Little change in the symptoms occurred until the beginning of August, when the cough became troublesome, dyspnœa increased, and he sweated freely.

For some weeks longer his state fluctuated between better and

worse. But on the 20th of September he was evidently very unwell. He had a slight rigor, he retched several times, and suffered intense pain in the left side and down the back. The temperature rose to 103·4°, the pulse to 108, and the respirations to 36. Later in the evening he experienced great distress from vomiting, and became slightly delirious.

Next day vomiting returned. The base of the right lung was found to be partially dull, and tubular breathing was heard. Posteriorly, on the left side, some crepitant râles. The sputum was not tenacious.

On the 22nd—*i.e.*, two days later—he complained of a choking sensation in the throat, and severe attacks of dyspnœa set in. He gradually became cyanotic, the pupils dilated, and he died on the 25th after a distressing struggle.

Post-mortem examination eight hours after death. *Rigor mortis* scarcely marked.

Abdomen.—Large intestine immensely distended. Liver normal size, but remarkably pale and soft.

Thorax.—Left pleura adherent over the greater part of its extent. Right pleura free except for a few bands posteriorly.

Large sub-pleural hæmorrhages over base of right lung. Towards anterior edge of its upper lobe was seen a wedge-shaped, white, cheesy mass, with its base outwards. Scattered through the same lobe were several small, dark, wedge-shaped nodules. Base of right lung congested and nearly airless, but did not sink in water.

A large, solid, ovoid mass was seen occupying the middle and upper part of left side, and below it the heart lay twisted transversely. The tumour was attached behind to the spine, its posterior wall being deficient, and the bodies of the second, third, fourth, and fifth dorsal vertebræ were eroded—the intervertebral discs projecting; the vertebral ends of second and third left ribs were denuded of periösteum and eroded. The tumour was almost completely filled with firm, laminated clot. It sprang from the aorta about one inch above the sigmoid valves and involved the ascending and transverse portions of the arch. The aorta was but slightly atheromatous. The left subclavian artery was pervious, but flat-

tened. The left vagus was compressed into a ribbon-like band. The œsophagus was incorporated with the wall of the aneurysm; a number of small, white, warty growths projected from its mucous membrane.

The left popliteal artery and vein were fused together—the vein much thickened. The artery was completely occluded by an organised thrombus.

CASE OF UMBILICAL POLYPUS.

By C. B. BALL, M.D., F.R.C.S.;
Surgeon, Sir Patrick Dun's Hospital;
University Examiner in Surgery.

[Read in the Section of Pathology, November 2, 1888.]

I am indebted to Dr. Falkiner for the following case. I was asked by him to see a child, aged six months, who had a small growth at the umbilicus. It was deep red in colour, granular on the surface, and attached to the umbilicus by a narrow pedicle; the skin in the neighbourhood was excoriated and wet with secretion. Dr. Falkiner ligatured the pedicle and cut away the growth, kindly placing it at my disposal for examination.

A horizontal section through the centre of the growth showed that it was made up of the following tissues from within outwards:—(1) Bundles of muscular fibres arranged circularly and containing between them large groups of blood-vessels; (2) radiating muscular bundles, forming trabeculæ amongst the gland tissue; (3) gland tissue, containing dilated and tortuous vessels in the intertubular stroma. At several places in the section little masses of adenoid tissue—solitary lymph glands—were to be seen. A more detailed examination of the tubules shows that they consist of a duct opening by free orifice on the surface of the polypi, but deeper in the growth they are much-branched acini, and that the character of the epithelium differs markedly in the ducts and acini. In the former the cells are long, narrow, and columnar, with an oval nucleus in the long axis of the cell; while in the acini they are much broader, with a crescentic nucleus at the base of the cell—the whole arrangement resembling closely the pyloric glands of the stomach, and unlike the ordinary Lieberkühn follicles. These appearances are well shown in the drawings kindly made by Dr. Patteson.

In an interesting paper on this subject by Dr. Van Heukelom, he has collected a number of cases of this condition (*Archiv für*

Pathologische Anatomie, etc., Virchow, Band CXI., Heft 3, March 2nd, 1888).

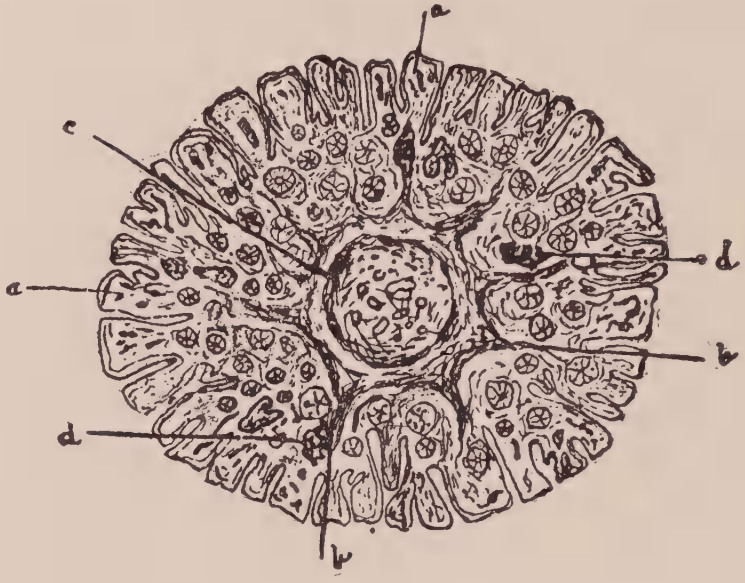


Fig. 1.

Semi-diagramatic view of tumour, showing general arrangement of muscular (central) and glandular (peripheral) parts :—*a*, Dilated and tortuous vessels lying in the inter-tubular stroma ; *b*, Radiating muscular bundles, forming trabeculæ among the glandular acini ; *c*, Circular muscular bundles, containing large central groups of vessels ; *d*, Nodules of adenoid tissue—solitary lymph-follicles.

In some the tissue is described as resembling Lieberkühn's follicles, while in the majority it appears identical with the pyloric glands, so much so that he describes the cases as "ectopia ventriculi at the navel," apparently rather an inappropriate name, as though histologically similar, they are manifestly unconnected with the stomach. What is the etiology of these remarkable growths? That they are portions of the omphalo-mesenteric duct, which has become cut off and involuted, would at first sight appear highly probable, and in the cases in which the epithelium resembles the intestinal gland this is a satisfactory theory. But how are we to account for the occurrence of pyloric glands in them? Van Heukelom advances a theory which appears to be more ingenious than plausible. He considers that the bile has something to say to it, and that Lieberkühn follicles only develop in those portions of the intestinal tube which are exposed to the influence of bile, and that in the stomach, and in those growths which are not exposed to bile, pyloric glands develop.

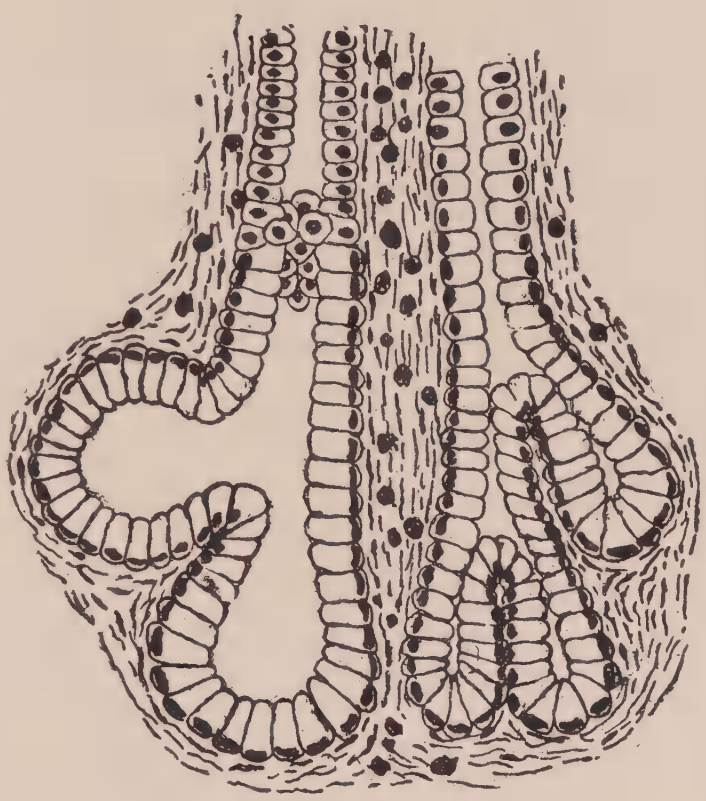


Fig. 2.

Showing ducts and acini, similar to the pyloric glands, and the fine, richly cellular connective tissue forming the inter-tubular stroma.

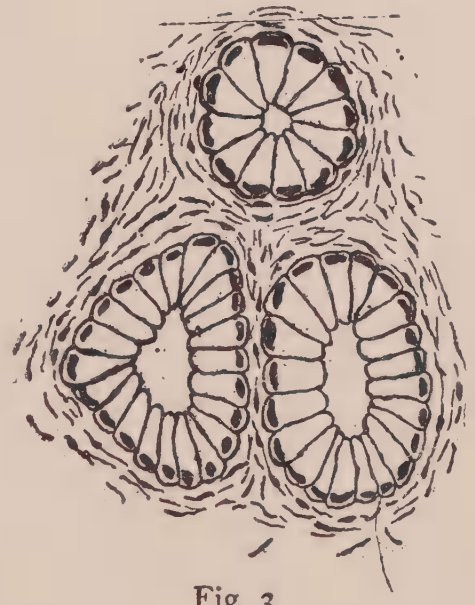


Fig. 3.

A group of acini cut transversely, showing the flattened crescentic nuclei of the epithelial cells.

A further point of interest is the secretion from these growths. This is noted in some of Van Heukelom's cases, but I regret to say that we omitted to make any examination of the secretion in the above case. It would appear, however, that the adenoid epithelium differs from that usually met with in pathological formations by being functionally active.

I believe these growths to be not very uncommon. I have met with two similar cases before, but, as I considered them only to be granulation tissue, I simply destroyed them with caustics. Although clinically unimportant, there is evidently much yet to be made out as to the etiology of these structures.

EPITHELIOMA IN THE HORSE: A NOTE IN COMPARATIVE PATHOLOGY.

By R. GLASGOW PATTESON, B.A., M.B.;
Late Surgical Travelling Prizeman, University of Dublin.

[Read in the Section of Pathology, November 30, 1888.]

The rarity of epithelioma among the lower animals and its frequency in man lends an interest to an undoubted example of its occurrence which intrinsically it hardly merits. In the horse especially it seems to occur but seldom. The leading veterinary authorities refer to it with merely a passing observation as to its rarity.

Fig. 1. × 50.

Professor Williams, in perhaps the most recent text-book, speaking of epithelioma in general, says:—"It is a very rare form in the lower animals; arises from some previous local disease or injury; and is seen more frequently in dogs than in the other domesticated

animals."[a] But the cancer which occurs in dogs, most commonly in the mammary gland of the bitch, is of the type known as scirrhous, or hard, cancer. Mr. Bland Sutton, after stating that he has met with only two undoubted cases of epithelioma among the lower animals, remarks:—"On more than one occasion penes have been shown to me which were removed from horses said to be affected with epithelioma, but the supposed cancers were only crops of soft warts."[b] In connection with this, Dr. M'Fadyean, Demonstrator of Pathology in the Royal Veterinary College, Edinburgh, informs me that he has only met with one example of carcinoma from the horse, and adds:—"I have several times examined tumours from the penis of the horse, but I always found them either fibrous or of the nature of gigantic warts."

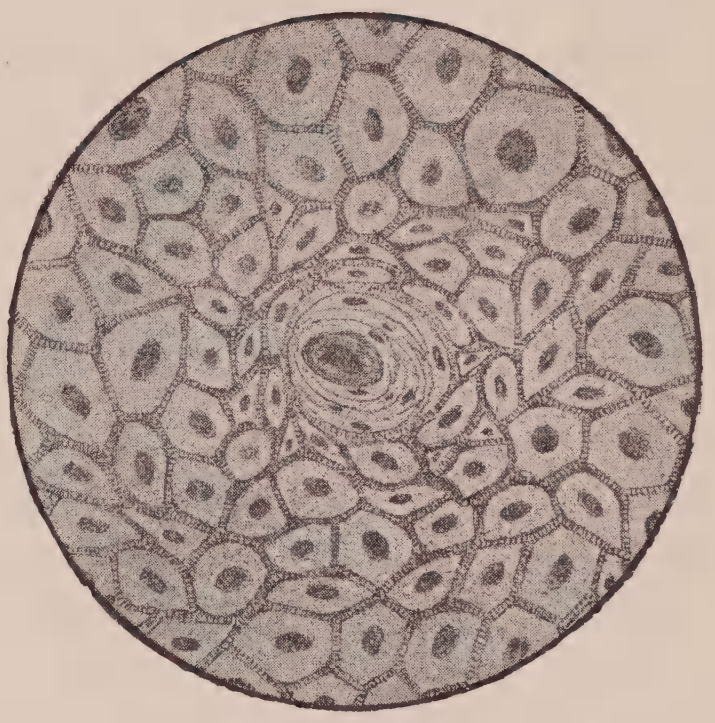


Fig. 2. × 400.

The horse the subject of this growth came under observation about a year ago. He had served mares for two seasons, had been

[a] Principles and Practice of Veterinary Surgery. By William Williams, F.R.C.V.S., &c. Fifth Edition. P. 447.

[b] Introduction to General Pathology. By J. Bland Sutton, F.R.C.S., &c. P. 318.

castrated as a five-year-old, and was then about a nine-year-old. The tumour, which resembled a large wart, occupied the inferior aspect of the penis, about two inches from the glans. It was removed in the usual way. Recurrence took place in about three months, the growth having then spread to the superior surface of the penis. It was again removed, and about three months ago it was found necessary to repeat the operation, the growth spreading rapidly from the seat of the old wound. There was no affection of the inguinal glands.[a]

The tumour sent to me for examination was about $2\frac{3}{4}$ inches in length and about $1\frac{1}{4}$ inch across the broadest part. The edges were hard and everted, the surface elevated and nodular, with commencing ulceration. Microscopical examination proved it to be a typical epithelioma. Fig. 1, drawn under a low power, shows the general arrangement of the cells penetrating deeply into the subcutaneous tissue, and numerous groups of concentrically arranged cells, the so-called epithelial "pearls."

Fig. 2, taken from a preparation mounted in glycerine, shows well the formation of a "pearl" in an early stage, and also the "prickle" cells characteristic of the stratum spinosum of the epidermis.

Another point of interest in the tumour is that although springing from a part the epithelium of which normally contains pigment, yet in no portion of the neoplasm could pigmented cells be found.

[a] For the details of the previous history of the growth I have to thank Mr. Gabbie, M.R.C.V.S., of Dundalk.

CONGENITAL MALFORMATION OF THE HUMERUS IN A MUMMY.

By EDWARD H. BENNETT, M.D., F.R.C.S.;
Professor of Surgery in the University of Dublin, &c.

[Read in the Section of Pathology, November 30, 1888.]

I am indebted to the Rev. G. T. Stokes, D.D., who has charge of Marsh's Library, for the opportunity of showing this curious specimen, which illustrates the unchanging character of two at least of man's diseases. This bone, taken from an Egyptian mummy, shows a congenital malformation, and also chronic rheumatic arthritis, absolutely the same in their character as we find them in the present day.

The greater part of the skeleton, with the mummy case and parts of the clothes, were found in re-arranging the library, having been probably brought to Dublin by one of its early custodians, who was known to have travelled in Egypt, and to have been interested in such curiosities. The bones of the extremities are complete, so that for our purpose the limitation of the disease to the single bone is established. In no other bone is there defect except in the scapular articular surface, which shows arthritic changes corresponding with those of the head of the humerus, and the bones of the forearm, which are a shade smaller and are of more slender build than those of the opposite limb.

Comparing the two humeri, the left is seen to be perfectly normal—that of a full-sized man; the right is very small and stunted, and at the elbow is remarkably altered from the normal—its width from condyle to condyle is greater than the corresponding measurement of the left bone, while the width of the articular surface is smaller. The shape and appearance of the bone is such that if it were placed by itself before one familiar with the

abnormalities of the skeleton, the first idea as to the cause of its stunted character, its rugged shape, and its abnormal width at the lower end, would be, I think, that it was a rickety bone; but this idea is at once put aside by its association with bones absolutely free of rickets in all other parts of the skeleton. Its arrest of development has not resulted from fracture, for there is no trace of such in any part of it.

In our collection we have a specimen which in all respects repeats these characters, which I myself dissected some years ago. This, for want of a better name, has been placed among congenital arrests of development. One idea suggests itself—that the arrest in these cases might be the result of infantile paralysis; possibly, but against this is the fact that in a specimen of this condition—of which I know the history—affecting the arm only, the changes are unlike those before us.

SYPHILITIC DEPOSITS IN THE HUMAN TONGUE, FOLLOWED BY EPITHELIOMA.

By W. I. WHEELER, F.R.C.S.; M.D., Dubl.;
Surgeon to the City of Dublin Hospital.

[Read in the Section of Pathology, November 2, 1888.]

The specimen which I shall show this evening is a portion of the tongue of a male patient, forty-eight years of age, who was under my care in the City of Dublin Hospital last year. He had served for twenty-one years in a foot regiment, and in the eleventh year of his service was unfortunate enough to contract syphilis, which was followed by an ulcerated throat, an eruption over the front and back of his chest, and some periosteal inflammation over his left tibia. Shortly after the partial subsidence of those symptoms a circumscribed swelling appeared on the left side of his tongue, rather towards the posterior part. For the above-named affections he was treated with mercury and iodide of potassium. The eruption disappeared. The periostitis yielded to this treatment, as also did the swelling on his tongue, but it never completely disappeared.

Three years after he had contracted syphilis the swelling of his tongue increased. Mercury and potash were administered, and again the enlargement reduced. On four subsequent and different occasions he was similarly treated, and on each occasion there was a marked diminution of this swelling, until the last occasion—a year before he retired from the service. On his admission into the hospital, the growth, as alluded to, could be both seen and felt—hard and firm at its edges, and having a granulated appearance on parts of its surface.

On account of his previous history I was at first inclined to believe it only syphilitic, but on closer examination I observed some small granular masses which are unusual in purely syphilitic

ulceration (although sometimes occurring). I therefore determined to excise it. Another reason that induced me to operate immediately was the fact that my experience has taught me that old syphilitic deposits are fertile sources for cancerous development.

This specimen, and the last case which I have operated on, is epithelioma. The microscopic sections, kindly made by Dr. Graves, exhibit a well-marked inflammatory process beneath the epitheliomatous growth, which does not extend far into the muscular structure of the tongue.

In confirmation of the fact that syphilitic deposits are fertile sources for cancerous development, perhaps in consequence of the injury that the structures incur from such deposits, I have brought here the specimens marked 1, 2, and 3, male and female tongues excised by me, which are examples.

The specimen marked 1 has a history very similar to that I have just recorded. It was microscopically examined by Dr. Abraham, who stated that the ulcerated surface was epithelioma, whereas the deeper and surrounding thickened portions were inflamed tissue and adventitious material.

The disease in specimen marked 2 originated in a syphilitic deposit. The female from whom I excised this tongue had borne two syphilitic children, and suffered from a syphilitic lesion of her tongue for seven years, which was noticed shortly after the primary infection. This specimen is epithelioma throughout the entire growth. No. 3 has no very accurate history other than that of syphilis contracted eight years previously to operation, and the growth, which is epithelioma, developed in a tumour of the tongue, which had remained quiescent for four years prior to the operation.

Those experiences which I have mentioned, and which I could have easily multiplied by additional specimens and similar observations, have led me to operate early in all persistent growths or deposits in the tongue, which I have shown are prone to develop into cancer, and consequently are better removed in what in my opinion has been correctly termed the pre-cancerous stage.

EPITHELIOMA OF TONGUE.

By A. H. CORLEY, M.D.; M.Ch. (Hon. Causâ), R.U.I.;
Fellow, Professor of Surgery, and Past-President, Royal College of Surgeons, Ireland;
Surgeon to the Richmond Hospital.

[Read in the Section of Pathology, January 11, 1889.]

The case which I have the honour of submitting to your notice this evening, together with the microscopic specimens and diagrams, illustrates the difficulties so often experienced in the diagnosis of lingual ulcers.

Case.—R. W., aged forty, was admitted to the Richmond Hospital, November 2, 1888. He stated that about twelve months previously he occasionally felt a stinging pain in a spot on the right edge of his tongue, about one-third from the tip. Some three months after this he discovered a round, hard lump the size of a pea in the same locality. After some weeks this became a sore, and when first seen, about fourteen days before admission, there was a superficial ulcer the size of a sixpence, the surface flat and a little above the level of the surrounding mucous membrane, and of a greyish colour. It resembled very much an exaggerated syphilitic mucous patch, but the closest cross-examination failed to elicit from the patient any history of syphilis. As he seemed a truthful man, and freely admitted having had a gonorrhœa, it was thought that if he ever had had syphilis it was one of those cases of mild primaries and secondaries which did not attract his notice, but which may be attended years afterwards with marked tertiaries. It was determined that he should be placed upon a short course of mercury, with mercurial fumigation locally, as an aid to diagnosis. This treatment was carefully carried out for about a fortnight, until the gums were slightly touched. The influence on the sore was not beneficial; on the contrary, the sufferings increased, and a severe pain in the ear, which had been intermitting, became permanent and much more

severe. The ulcer increased in size and lost the grey colour, becoming irregular on the surface, excavated and hardening at the edges. It occupied about half the anterior and half the middle third of the tongue, involving part of the under-surface, edge, and extending half way to the mesial line across the dorsum. As there was no doubt now of its nature, the ulcerous mass and the anterior two-thirds of the right half of the organ were removed, November 28, 1888, by a special operation. There was hardly any hæmorrhage, and the wound cicatrised completely in about six weeks. There was a good deal of "puckering" during this process, and the scar felt rather hard and lumpy when he left hospital, with his articulation little impaired.

The microscopic specimens prepared by my friends, Drs. Scott and Graves, exhibit in the centre of the ulcer a typical epitheliomatous formation. Around the sore for some distance are ordinary inflammatory changes of tissue, and multiplication of leucocytes. The large drawing, which exhibits a complete section of the right lateral half of the tongue seen from behind, is a coloured photographic enlargement of a transverse section made through the centre of the ulcer. The gradual change from the normal thickness of the sub-lingual and dorsal epithelium to its marked increase at the margins of the ulcer, and the irregular dipping in and its infiltration amongst all the proper tissues—connective, glandular, and muscular—is well shown at the most excavated portion of the ulcer.

I think this case presents features of great interest from a pathologically-diagnostic point of view, and I am sure the illustrative microscopic sections will add to its value as a clinical record. It shows the difficulty of ordinary diagnosis between syphilitic and cancerous disease of the tongue, even when there is no history of antecedent syphilis. I did not expect any distinctly pathognomonic results from the negative effect of the mercury, although its positive effect would have given them. If a syphilitic sore had in the progress of time become cancerous, the same absence of result would be natural and yet not negative the pre-existence of syphilis. At the present time when the increase of cancer has been so great and widespread through the United Kingdom that it is beginning to

arouse the attention of our legislators, it behoves us to observe and study with the closest attention every case likely to throw light upon the origin of this dreadful disease. A good deal has been recently written upon the connection between syphilis and cancer, and in my own experience I have seen many cases, especially of lingual cancer, where syphilis had been present, but I have strong doubts as to the real identity, correlation, or interchangeability of the two diseases. That syphilis may predispose to cancer I think admits of little doubt, but the same may be said of other chronic depressing maladies. As regards the lingual ulcers I am inclined to believe that the chronic syphilitic sore acts as a local irritant, just as the stem of a short pipe does in the case of the lips, or the soot in that of the scrotum, and that the cancer is the result of the irritation, not of the specific disease, just as a sore may have been at first a simple ulcer produced by a jagged tooth and subsequently have become cancerous. As there is a strong impression, although yet unproved, that both the diseases have for their real essence the presence of a microbe, the question arises—is it, if present, the same organism in both?

A CASE OF ENDOTHELIOMA OF THE LIVER.

By ALEXANDER B. McKEE, M.B., Dub.;
Curator of the Museum, Royal College of Surgeons in Ireland.

[Read in the Section of Pathology, January 11, 1889.]

Were it not that the interest of the case forming the subject of this communication is exclusively histological, some apology should be offered for the unavoidable absence of a full account of the clinical history and *post mortem* appearances. For the few details which I am able to give I have been compelled to trust to memory, but they are, I believe, tolerably accurate.

The patient, a woman of about fifty years of age, suffering from the usual consequences of portal obstruction, was under Professor Kussmaul's care, in the medical clinic of the Bürgerspital, Strasburg. The only notable point in her family-history is the death of two of her sisters from ascites. She had given birth to several healthy children, and had herself enjoyed good health up to the date of that illness. Tapping of the abdominal cavity, which was enormously distended with fluid, allowed the contour of the liver to be distinguished. The organ was found greatly enlarged, and the discovery of a hard, prominent mass of considerable size in connection with its left lobe gave rise to the belief that the case was one of malignant tumour. A fatal issue was accordingly anticipated, and palliative treatment adopted. Her death occurred some three or four months after her admission to hospital.

The *post mortem* examination was made by Professor Von Recklinghausen. Except in the liver, the changes revealed were few, and chiefly of negative importance. There were signs of chronic pachymeningitis and of chronic interstitial inflammation of one kidney. The peritoneal cavity contained a large quantity of fluid having the usual ascitic characters, but no fibrin. The portal vein

was free from thrombi or other abnormal contents; the glands in the fissure were natural. Though a careful search was made, no sign of tumour-formation could be discovered in any part of the body save the liver. This organ was greatly enlarged, and must have weighed 7 or 8 lbs.; it was smooth on the surface, more than usually consistent, and reddish-brown in colour. The prominence noticed during life in connection with the left lobe was found to consist simply of gland-substance. Section showed a general and marked increase of the interstitial tissue, and, in addition, several nodules scattered throughout the organ. These varied in size, some being as small as a pea, soft, vascular, and indistinctly defined; and the largest as big as a cherry, hard, fibrous-looking, sharply circumscribed, and poorly supplied with vessels. To the naked eye, therefore, the case appeared to be an example of the not uncommon combination of carcinoma with cirrhosis.

Professor Von Recklinghausen very kindly gave me the organ for microscopic examination. The first look at a small nodule hardened in spirit seemed to confirm the diagnosis. In preparations treated with picro- or with alum-carmine, groups of cells arranged in alveoli presented a marked contrast to the surrounding less brilliantly stained liver-substance, while a very decided increase of the interstitial connective tissue was apparent. But this superficial resemblance to carcinoma of epithelial origin was not borne out on more careful examination, nor did the interstitial changes exactly correspond to those commonly seen in cirrhosis.

In the smaller more vascular nodules, the arrangement of the groups of cells composing the new growth appears in general to have been determined by the disposition of the capillary network of the lobule. The form of the lobule is still indicated in places, even where the liver-cells have completely disappeared, or have been reduced by atrophy to a mere vestige—in fact, in favourable preparations it is always possible to make out that the cell-groups lie in every instance in enlarged capillary vessels, as proved by the existence of an endothelial lining to the spaces, and (in preparations hardened in Müller's fluid) by the presence of red blood corpuscles (Fig. 1, *e*; Fig. 2, *e*, *b*). Even where their situa-

tion cannot readily be distinguished there is no reason whatever for believing that they are located outside the vessels. The clusters of cells do not, however, in the vast majority of instances, lie free in the vascular lumen, but are continuous with the extravascular tissues at one side (Fig. 2, *a*); they project into the vessel, and by their free margin limit an area the opposite boundary of which is formed by the capillary wall. Sometimes there is only a single row of cells, occupying the site of the normal endothelium, for which they appear to have been substituted. At others, the cells are placed three, four, or more deep; but in most groups they are seated upon a central mass of connective tissue, which, apparently, had grown into the vascular lumen, carrying the cells before it.

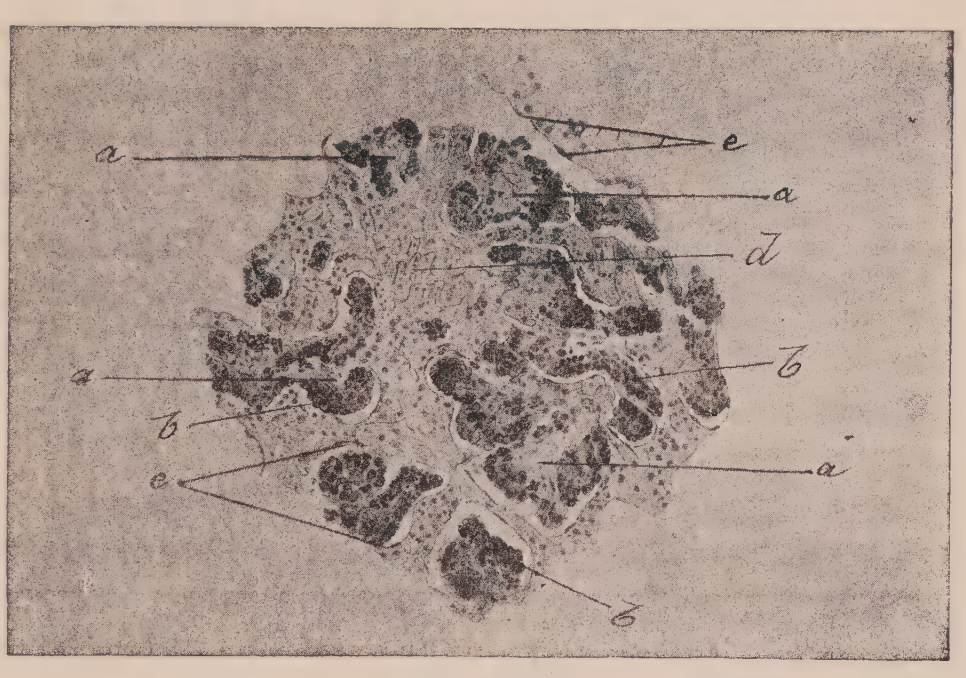


Fig. 1.

From the Periphery of a small Nodule, hardened in spirit.
a, Connective Tissue Core.
b, Cells of New Growth arranged in Clusters.
d, Hepatic Substance.
e, Endothelium of Capillaries. (Seibert, ob. ii., oc. 1.)

These differences may, at least in part, be explained on the supposition that the cells are disposed somewhat like the drupels of a raspberry or blackberry, around a fibrous core corresponding to the receptacle. Sections made through the fruit would consist,

according to their direction, of drupels and receptacle, or of drupels alone. On this view the clusters must have grown *in situ ;* and it is impossible to escape the conviction that they actually did so, though in rare instances appearances are seen, such as those represented in Fig. 3, *a*, *b*, where the cell elements must have been carried by the blood-stream into the situation they occupied at death. Bearing in mind that the affected capillaries present, in part of their circumference, normal endothelium, and in another part, the cells of the new growth, which project naked into the lumen, it must, I think, be considered probable that these cells were derived from the endothelium itself. It appears certain, at any rate, that we are not dealing with a cancer of *epithelial* origin.

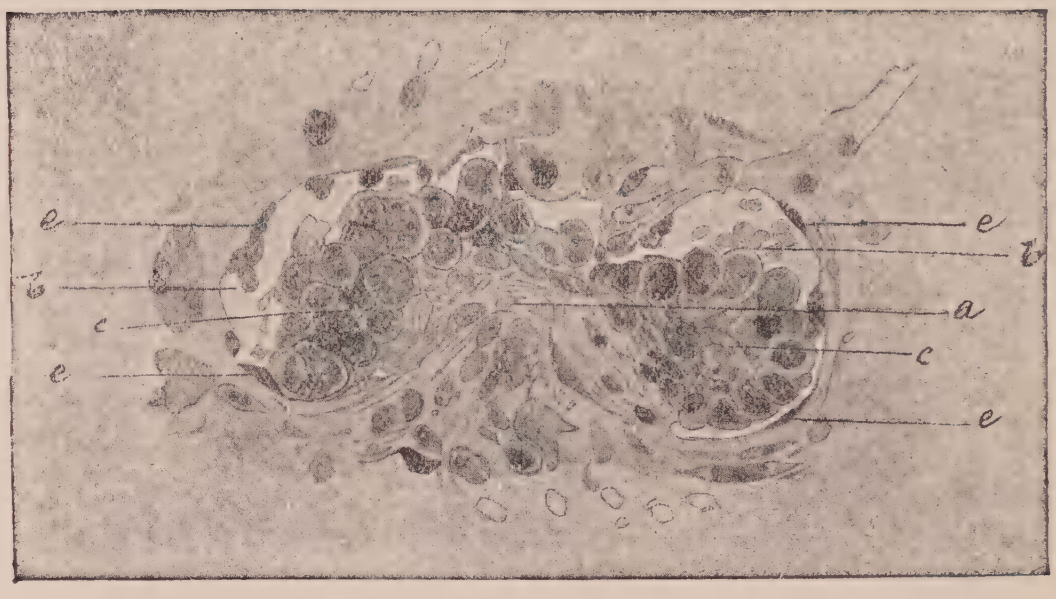


Fig. 2.

From a larger Nodule, hardened in Müller, in which the fibrous changes were more advanced.

a, Connective Tissue carrying the Cells.
b, Red Blood Corpuscles in Capillary Lumen.
c, Cells arranged in Clusters.
e, Endothelium of Capillary. (Hartnack, ob. viii., oc. 3.)

This conclusion is still further strengthened by the characters of the cell-elements. By far the greater number of these are round or oval bodies, having a diameter of 10·5—15·8 μ ($1\frac{1}{2}$—2 size of a white blood-corpuscle), and containing a very large nucleus

which fills almost the entire cell. Smaller cells are occasionally met with, and, more rarely, multi-nucleated forms, almost deserving from their size to be ranked with giant-cells. Two nuclei are sometimes present in the cells of average dimensions; and in these and in the largest cells we often find appearances indicating completed segmentation of the nucleus (Fig. 3, *c*).

Fig. 3.

Cells seated in undilated Capillaries from different parts of the Liver outside the Nodules.

a, *b*, carried by Blood Stream.
c, *d*, seated on Wall—of doubtful origin. (Seibert, oil. xii., oc. 1.)

In what are presumably the youngest portions of the growth a considerable amount of fairly normal hepatic substance may here and there be seen lying between the cell-groups (Fig. 1, *d*). But even then the liver-cells are generally smaller than natural; while in most parts of the nodules they are greatly distorted and atrophied, or have completely given place to connective tissue. Where this fibrous change has occurred, the core, if it exists, is also more condensed and sharply defined against the cells seated upon it. Indeed, so greatly does the connective tissue encroach upon the cell-elements, not only of the liver, but of the new growth as well, that ultimately the capillaries containing it are

obliterated, and the cells themselves caused to disappear by pressure-atrophy. The successive steps of this process can be traced in different portions of the nodules, its final stage being represented by the large fibrous tumours noticed at the *post-mortem.*

On examining one of the latter microscopically, it is seen to consist of dense connective tissue, containing a few nuclei scattered throughout it. Vessels are noticed at intervals, and in some places can be made out to consist of veins, arteries, and, apparently, altered bile-ducts, grouped together. Though the intervals at which these groups occur are not very regular, they are yet sufficiently so to give one the impression that the vessels are those of the inter-lobular connective tissue—the sole survivors of the destructive process. It is naturally more difficult to identify the intra-lobular veins, but most, if not all of them, seem to have disappeared.

This obliteration of the central veins, which was observed not only in the fibrous, but also in the softer nodules, as well as in the comparatively unaltered portions of the liver, gave rise to the idea that the entire process might have started in these vessels. With a hope of determining this point, the hepatic vein was followed back with scalpel and scissors to its smallest sublobular tributaries, and thin microscopic sections made, so as to divide the walls of the central veins longitudinally. Appearances such as those somewhat inadequately represented in Fig. 4 were then met with; and though no direct evidence of proliferation is here apparent, still we find cells which occupy the site of the endothelium, and which certainly may have been derived from it. It is unlikely, however, that the process originated exclusively in these veins, for similar appearances were observed, though more rarely, in the minute ramifications of the portal—not only so, but in the larger branches of the portal and hepatic veins a few small nodules, about the size of a pin's head, were discovered sitting on the vessel-walls. Microscopic examination proved that these also consisted of a central core of connective tissue, bearing the cells upon it.

So general were these appearances in the various portions of the venous and capillary circulation of the liver that the question arose

whether the formation of the nodules and the diffuse cirrhosis-like changes might not fairly be referred to one and the same process. In the common form of cirrhosis, as is well known, the production of connective tissue starts, and continues to preponderate, in the periportal areas, and spreads thence along the capillaries of the parenchyma, in which fibroblasts make their appearance, and from which an emigration of leucocytes takes place. Less commonly, as in the present case, the production of connective tissue is more general, and the lobules are not mapped out by the interstitial hepatitis. It became necessary, therefore, to attempt to discover whether the connective tissue was produced in the ordinary way, or whether the cells of the new growth themselves contributed to its formation.

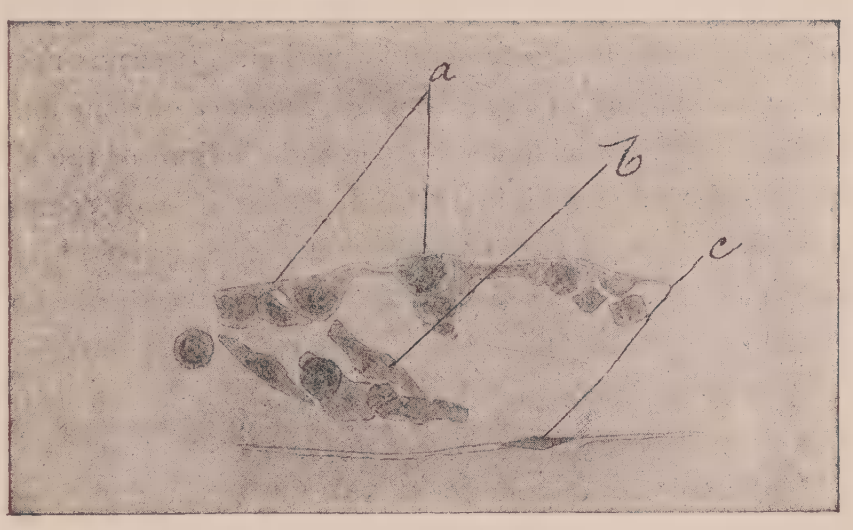


Fig. 4.

Portion of a Central Vein near its entrance into a Sublobular Vein.

a, Large Cells occupying Site of Endothelium.
b. Cells lying free in Lumen (Fibroblasts ?).
c. Endothelium. (Hartnack, ob. viii., oc. 3.)

During a lengthy and very careful examination no certain evidence could be obtained that these cells behaved otherwise than passively. Some, provided with a single long process, or with two processes, one at either end, were seen, but no fibrillation. Others, seated on the fibrous cores, sent down into them long extensions, but in no other way did they contribute to their formation. So far as could be discovered, then, the diffuse changes

do not differ in kind, but only in distribution, from those commonly seen in cirrhosis—with this exception, that clusters of cells similar to those of the nodules are found at intervals throughout the organ.

As regards the relation of the inflammatory process to the new formation, no opinion can be given. Both may have been due to the action of some irritant circulation in the blood, or—though this is less probable—the intra-capillary cell-clusters may have been produced secondarily, as the result of an attempt on the part of the interstitial connective tissue to penetrate the capillary vessels, a proliferation taking place—such as occurs, for example, in the epithelium, in some cases of cystic adenoma of the breast. But bearing in mind the circumscribed nodular character of the new growth, it is difficult to see how either a diffuse hæmatogenous hepatitis, or a poison conveyed into all parts of the liver by the blood-stream, could *alone* give rise to the tumour formation. It seems necessary to assume the existence of a predisposing cause, or of some unknown factor which acted coincidently on the endothelium of the affected portions of the organ; and in this connection it is of some slight interest to recall the fact that two of the patient's sisters had died with ascites. The same consideration—the circumscribed nature of the growths—makes it impossible to regard them as forming an integral part of the inflammation; again, the elements of the tumour are neither round cells nor fibroblasts, nor, as far as could be discovered, did they take any active part in the production of connective tissue. Moreover, this consideration appears to remove the growth from the category of atypical cell-proliferation, such as occurs in the anterior epithelium after wounds of the cornea, in the epidermis in elephantiasis and lupus, and elsewhere, though it must not be forgotten that isolated cell-groups identical with those of the nodules were found in various parts of the liver, and may have originated there. The claim of the nodules to rank as true tumours will, at any rate, be freely admitted by those who adhere to the *anatomical* conception of the term.

But whatever be the essential nature of the process, that the growth was primary and confined to the liver, was proved *per exclusionem* at the *post-mortem*. That the larger, harder, poorly

vascular, fibrous nodules were not perhaps scirrhous, and did not give rise to the smaller more vascular ones by metastasis, was satisfactorily demonstrated by the occurrence of transitional stages, and also by the structure of the small nodules themselves. No proliferation was observed in the epithelium either of the parenchyma or ducts. The arteries were normal; so that the only tissues that remain to be considered are the interstitial, and the walls of the veins and capillaries. Either of these might, by their proliferation, give rise to elements having the characters previously described; but in no place do the capillary walls appear to have been penetrated; the walls were at most pushed forward into the lumen.

If I am correct in my belief that the new growth originated in the endothelium of the veins and capillaries, this is certainly a very rare tumour of the liver. In a prolonged search through the literature of the subject I failed to discover anything whatever resembling it; nor had Professor Von Recklinghausen, in his vast experience, ever seen a similar case.

For sake of completeness I may enumerate some other appearances, which, as being unessential, have been reserved until now. The atrophic liver-cells, either single, or arranged in groups or rows in the midst of the newly-formed connective tissue, are abnormally pigmented, but appear to contain fat only where the capillaries have continued pervious; there was no fatty degeneration. Collections of round cells may be seen here and there throughout the dense connective tissue. The capillaries are often greatly dilated, even when free from the new growth, and groups of dilated capillaries give to some portions of the organ an almost angiomatous aspect.

To Professor Von Recklinghausen my thanks are due, not only for placing this valuable material at my disposal, but for looking through all my preparations. I regret that I have not had the benefit of his criticism of this paper, for the opinions expressed in which I alone am responsible. Professor Von Recklinghausen aided me by his criticism and advice throughout the examination; and for this, and many other favours, I desire to express my deep sense of gratitude.

A CASE OF INTRATHORACIC TUMOUR.

By JAMES LITTLE, M.D., F.K.Q.C.P.;
Physician to the Adelaide Hospital.

[Read in the Section of Pathology, January 11th, 1889.]

The specimen is chiefly valuable in relation to the diagnosis of intrathoracic tumour. Towards the end of last month a man, aged forty-one, came to the Adelaide Hospital, saying that he had lost a good deal of flesh, although he looked a tolerably healthy farmer. He stated that twenty years previously he had suffered from pleurisy on the right side, and had been aspirated. After that he enjoyed good health until the commencement of his present illness, three months previously. At that time he thought he had caught a cold, as he began to cough, and occasionally brought up sputum slightly tinged with blood. He also noticed that while his breathing was tolerably easy so long as he walked about or sat on a chair, it became oppressed when he lay down. Beyond these thoracic symptoms he had no indication of illness, except that his appetite had failed a good deal, and that he had lost flesh. When we examined his chest we found first the evidences of the old pleurisy. The right side of his chest was manifestly contracted. The next thing that struck us was that the veins of the upper extremities on the left side were rather full. There was stridor present. When we percussed him we found that while there was a full resonant sound in the upper part of the left chest, there was stony dulness, with complete absence of resiliance, in the super-mammary region on the right side. When we listened over that region there was an absence of the breath-sounds, except just under the clavicle, where there was a tubular breath-sound with prolonged expression. On examining his back on the right side we found certain amount of dulness on percussion over the lower part, but

that seemed to be accounted for by the old pleurisy. The respiratory sound was feeble, except just in the scapular groove, where there were prolonged and tubular expression. On the other side there was simply old puerile breathing. He lived for about three weeks in the hospital. He did not, for the greater part of that time, appear at all worse; but about three days before his death he was evidently a great deal worse. He could no longer lie down, but had to sit up and lean forward. I examined him the day before his death, and found that the physical signs had undergone a considerable change. On the left side there was no longer the puerile respiration, and there was clearness on percussion; on the contrary, there was feeble breath-sound on that side; and prolonged tubular expression was audible in the left groove as well as in the right. On the day before his death we observed the veins in his neck and arms to be rather full, but there was nothing of that great rigidity of the neck and extreme fulness of the veins that one expects to find in a case where there is great pressure on the descending cava or on either of the innominate veins. This was one of the puzzles that the case presented, having regard to the appearances that we found after death. There could have been very little doubt as to the diagnosis, even when he first came to the hospital. The stony dulness on percussion, the entire absence of impulse or murmur, and the stridor, could hardly have depended on anything except a solid tumour in the thorax. I felt no doubt as to the diagnosis myself, partly in consequence of the curious fact that eighteen years previously a man lay in the very same bed whose case presented precisely the same problem. The *post-mortem* appearances in that man's case are recorded in the "Pathological Transactions," and are referred to in Dr. Bennett's work on "Intrathoracic Tumours." The man whose appearances I now present had the same characteristics as the patient who occupied the bed eighteen years before; and I may mention them because they help to distinguish between intrathoracic growths and arterial tumours in the thorax. In these two classes of cases there is almost invariably a difference between the mental conditions of the patients. A man with an aneurysm which gives rise to difficulty

of breathing, swelling, or any such symptom, is usually impatient and anxious for relief. A man who has a solid, malignant growth, is usually apathetic and resigned. Another point, to which attention was drawn by Dr. Stokes, and which this case verifies, is, that in the progress of intrathoracic cancer there is no temporary amelioration. The progress may be slow or it may be rapid, but it is always steadily from bad to worse; whereas, a man with an aneurysm, in consequence of the fact that the aneurysm presses sometimes in one direction, and sometimes in another, obtains from time to time temporary relief from his symptoms. A third circumstance, which struck me in both of the two cases, was, that although there was so much distress from pressure on the respiratory tubes, there was very little pain. With an aneurysmal tumour coming so far in front of the chest as to give rise to dulness on percussion, and of such magnitude as to press so much on the air-passages, there would also be, in all probability, more or less pain from pressure on the intercostal nerve. A fourth circumstance was that with a solid growth such as there was in this man's case, the amount of relief the patient obtains by change of posture is always very small; whereas in aneurysm a man obtains no inconsiderable amount of relief by throwing his body into certain postures. The remaining point, to which I drew attention when showing the other case long ago, is the very temporary relief which blood-letting affords when we have to deal with an intrathoracic solid tumour, as compared with the great relief that may be obtained from time to time by the application of leeches to the surface of the chest when we have to deal with an arterial tumour.

After the death of the patient in the present case we found the appearances which I now present. First you have, occupying the greater part of the right lung, a growth, even the naked-eye appearance of which sufficiently establishes its character as a malignant growth. It occupies the greater part of the upper and middle lobes of the right lung. The modes in which the great tubes within the thorax were affected by the tumour are very different. The aorta and its branches were completely unaffected, though closely adherent to the growth. The same may be said of the

œsophagus, which is unaffected, though adherent to the tumour. It is not pressed upon or in any way obliterated. Very different was the relation of the air-tubes to the growth. The right bronchus, as may be seen in the specimen, is entirely encircled by the growth. The trachea is pressed upon by the growth; and when we look at the specimen posteriorly we see that the tumour pressed also on the left bronchus. In all probability it was the pressure of the growth on the left bronchus and trachea which gave rise to the difference in the physical signs during the last two or three days of his life, as compared with the previous signs.

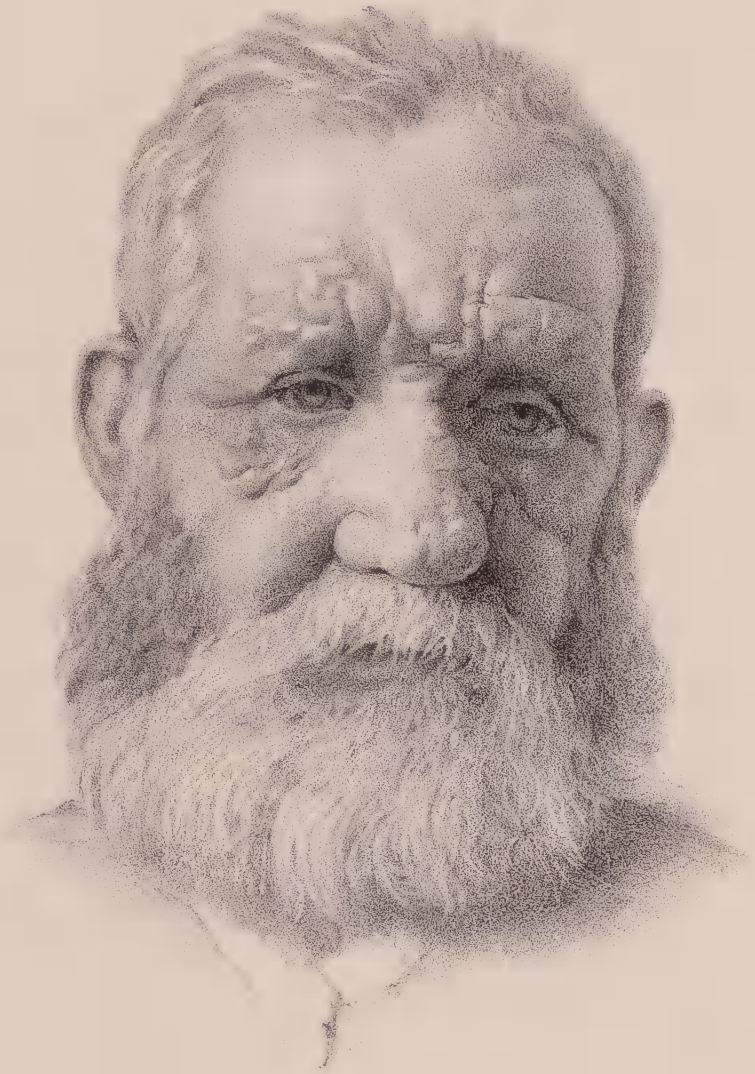
The most interesting point about the case, however, is the condition of the great upper cava. On making a *post mortem* Dr. Bewley found the descending cava completely obliterated by the mass which you see. A catheter will not go through it. At first it appeared as if the clot was only of a *post-mortem* character; but on examination Dr. Bewley found that it presented the same histological elements that appear in the tumour itself—namely, those which are characteristic of round, solid, sarcoma. He also noticed the extreme fulness of all the abdominal veins. All the abdominal viscera were extremely full of blood. He noticed that the azygos veins and the ascending cava were unusually capacious. It is difficult to understand how the blood coming from the head, neck, and upper extremities could ever have found its way through the azygos veins from the descending cava, and make its way up to the heart by such a circuitous route. At the same time it is certain that twenty-four hours before the man's death, beyond a somewhat unusual distinctness of the veins in the neck and upper extremities, there was none of that extremely venous turgescence that we generally see when the large vein leading to the right side of the heart is pressed upon. I have no doubt that this growth originated in the mediastinum, and that it only invaded the lung. Though I have seen several cases in which a diagnosis of pulmonary cancer was made during life, I have never yet seen a case of cancer of the lung verified by *post-mortem* examination, except where the cancer was mediastinal and spread into the lung.

A CASE OF LEPROSY.

By JOSEPH O'CARROLL, M.D., M.K.Q.C.P.;
Physician to the Richmond, Whitworth, and Hardwicke Hospitals.

[Read in the Section of Pathology, February 15, 1889.]

My justification for bringing this case before the Section is the rarity of leprosy in these countries, and especially in Ireland. The man, J. M., whom I show this evening, was admitted into the Whitworth Hospital, North Brunswick-street, on the 7th August, 1888. I have obtained the following history, partly from himself and partly from official sources of information:—I can find no trace or hint of leprosy in his family. His father, who had served through the Peninsular War, in the 11th Foot, died at an advanced age in Cork-street Hospital, some time between 1836 and 1840, of "throwing up of blood." J. M. himself was born about 1832; joined the army from the Hibernian Military School in 1845, as a drummer in the 73rd Regiment; went, about the end of 1845, by way of Rio Janeiro and Monte Video, where there was some fear for the safety of British residents, to the Cape; served there till about 1857, including the Kaffir Wars of 1846-47 and 1850-51; then in Bengal from 1857 till 1861, when he returned to England. He remained in these countries for the succeeding five years. In 1866 he sailed for Hong Kong. After about three years there he spent two or three months at Singapore, and was then stationed for some two years in Ceylon. In December, 1871, he was invalided to Netley. He arrived at Netley on the 24th April, 1872, and was discharged to duty after two months in that hospital. Two months later, that is, in August, 1872, he was finally discharged from the army. His military service is thus marked out into two periods spent abroad, separated by a stay of over five years at home.



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His medical history is as follows:—He is positive that he has never suffered from *any* form of venereal disease. I am the more inclined to believe him, as I can find no trace whatsoever of disease in the usual sites (with the exception of an enlarged testis, to be mentioned hereafter), and as, moreover, he attributes his immunity more to good fortune than to merit. In India he suffered several times from malarial fever. While in England and Ireland (1861–66) he enjoyed good health. Shortly after his arrival in Hong Kong he had on the front of his chest an eruption, the first he ever noticed on his body. In Ceylon he was vaccinated; he continued to have the ague there; and he had left orchitis, caused, he says, by a kick from a drunken comrade whom he was assisting to carry to the guard-house. For this and for debility he was invalided to Netley.

I wish here to express my thanks to Dr. William Carte, J.P., of the Royal Hospital, Kilmainham, and to Surgeon-Major Babington, Secretary to the Principal Medical Officer, Victoria Hospital, Netley, for their courteous and valuable help in making out the details of my patient's history since his arrival at Netley.

A note of his case, made ten days after his arrival at Netley, records "an eruption of coppery-coloured, dry, scaly spots" on his chest, and a few such spots scattered over the legs and arms. The note appears from its form to have been made by one of the Netley probationers, who, taking the rash, which he calls "psoriasis guttata," and the orchitis together, diagnoses syphilis, and the case appears to be entered in the Netley records as one of syphilis. After two months in Netley and two months at Manchester, spent mainly in the Military Hospital there, he was finally discharged as suffering from "general debility." For the next two and a-half years he was able to do easy work as light porter in a public-house. In February, 1875, he was admitted to the Royal Hospital, Kilmainham. On the 1st November next year he was placed in the Infirmary, suffering from an eruption which seemed of herpetic character. Later on, Dr. Carte tells me, on several occasions in 1882–83–84, the cuticle was raised by an unhealthy serum, and then followed sloughing of the underlying tissues. This was con-

fined to the hands and feet. He left the Royal Hospital in January, 1887. The man himself tells me that early in his stay in the Royal Hospital he had "ringworm" on the inner aspect of one thigh. Later on he noticed similar eruptions elsewhere. He came to learn of the eruption on his back only from the observations of those who saw him stripped. He fixes the date of the first appearance of the bullous eruption, to which Dr. Carte's note refers, as the 15th August, 1882, the day of the unveiling of the O'Connell Monument. He was about the city that day for several hours, and in the evening noticed a large bulla on the dorsum of his right foot. Since then such bullæ have appeared, from time to time, to the number, probably, of a hundred or more. About three years ago his feet became swollen, and since then they have not been free from one or more sinuses, which have greatly impeded him in getting about. During about the same period his hands have become crippled.

In describing the morbid phenomena of the case, I shall take them in an order in which they conveniently arrange themselves, namely—(*a*) eruptions, (*b*) paralyses, (*c*) atrophies and necroses.

With regard to the *eruptions* which he presents, we have to note their form and their site. Without going into fine distinctions, they may be described as of three types, namely:—

α. Discrete tubercles, reddish-brown in colour, some of them involving only the skin and some the subcutaneous tissue also. Some of them, indeed, appear to occupy a larger area beneath the skin than in it. At present they are more numerous on the forearms than elsewhere. They are not symmetrical in distribution. This form appears to precede, and in some cases, by coalescence of the tubercles, to merge into the next form.

β. Tuberculo-macular, often circinate eruptions. The face has generally a lumpy look; the elevations are not symmetrical; they pick out the eyebrows, cheeks, nose, margin of under lip, and scalp, giving the face the characteristic appearance which has been named *leontiasis*. Over the back there are patterns which help us to realise the "ringworm" which he had on his thigh many years ago. They consist of raised red margins, fading away into the sound

skin outside, unless where they are fused with the advancing margins of other patches, or with new tubercles. Their inner edge is sharp; it has a light, healthy-looking zone just internal to it; while within that again there is a purple-red patch of flat skin; and in some cases this central patch is again divided into dark and fair, more or less concentric, zones. It seems as if, like ringworm, the disease had pursued a centrifugal course, the raised margin being the advancing infective border, the central area the part which the disease has already passed over. But if this be so, this latter part is not protected from fresh infection, for in some of the central areas new tubercles are to be found.

On the front of the chest he has similar circinate patches; on the front of the abdomen, also, only here the pigmentation is of a yellowish hue. This eruption extends down the arms, as far as just below the elbows, in the neighbourhood of which it is thicker, that is, more nodular than anywhere else, and also down the legs. It has not involved the hands or feet.

I shall venture to call these two forms of eruption together the primary eruption. One seems to be merely a development of the other. It is interesting to note that this primary eruption has not hitherto involved the skin of the genitals, nor that of the eyelids, and that it is not, in the accurate sense of the word, symmetrical. It has extended into the nose, especially on the right side, and is visible in the pharynx as light, cream-coloured, or pearly patches.

γ. The third form of eruption is a pemphigoid one. The first appearance of this was, as I have said, on the 15th August, 1882. On that day, after several hours spent walking about, he noticed a large bulla on the dorsum of his right foot. Since then a great number of such bullæ have appeared, all on his extremities. Some of them have left no traces; the site of others is shown by a superficial glossiness, or cuticular atrophy. Of those which I have seen some dried up without ulceration of the cutis vera, while others burst, disclosing a superficial ulcer, which healed like an ordinary healthy ulcer. A remarkable peculiarity of these bullæ is the rapidity of their development. The patient tells me that he has frequently seen a bulla upon a spot which an hour previously had

been quite normal-looking. And since he has come under observation I have seen several bullæ matured, if I may say so, both as to size and contents on spots which were clear the day before. As to size, the bullæ vary from about half an inch in diameter to two inches. As to contents, some are serous and remain so; others pass through a serous, a sero-purulent, or a sero-sanguineous, and a fibrinous or flaky stage. In other words, this eruption may be set down as a pemphigus of chronic character, unattended by any special depression or debility.

The site of these bullæ is somewhat remarkable as contrasted with that of what I have called the primary eruption. So far as I can learn they have appeared in the upper limb, always on the ulnar side of each hand and on the back of the left one; and in the lower limb, always along the outer side of the leg and foot, but at both sides of the knee. That is to say, that the distribution of the bullous eruption is, in this case, more symmetrical than that of the primary one, and coincides with the course of nerves which are affected by disease. The bullæ have not appeared in areas in which the primary eruption is established; while they do appear in those parts which are most anæsthetic and in positions which are perhaps most exposed to continuous pressure.

His *paralyses* are both motor and sensory. His hands present the condition characteristic of paralysis of the ulnar nerve. The palm is flat and wasted; the thumb abducted and having its proximal phalanx extended and its distal flexed; the fingers are similarly extended at their proximal and flexed at their two further joints. The interrupted current has hardly any appreciable effect on the muscles of the palms or on the peroneal and anterior muscles of the legs. The muscles of the soles of the feet do not react to the interrupted current; there is a suggestive distortion of the great toe which is adducted towards the axial line of each foot. The second toes were thereby pushed upwards into such an awkward position that they were amputated in another hospital.

Sensation is dulled over almost the whole trunk. In the limbs anæsthesia is specially marked in the distribution of the ulnar and external popliteal nerves. In the right hand the median nerve

area is also anæsthetic. This anæsthesia holds equally for ordinary touch, pain, heat, and electricity. The patient's muscular sense, unless in these areas, appears to be normal. It is interesting to note that he is not anæsthetic in the cornea, nose, external ear, pharynx, soft palate, or lower part of rectum. In the right leg, at the junction of the upper and middle third on the outer aspect, he has a patch of deep hyperæsthesia. Less marked hyperæsthesia is to be found, on deep pressure, here and there in some of the superficially anæsthetic areas of the limbs. In the arms, where the tubercular infiltration of the skin is thickest, the tubercular patches are more sensitive than the adjacent sound skin on the side further from the centre of innervation.

The *necroses* and *atrophies* which he presents may be summed up shortly. He has caries of some of the phalanges of the feet, some ulceration of the nasal mucous membrane, and a small ulcer of the palate developed within the past fortnight. His right little toe came off, he tells me, in a poultice; on one occasion some bone came from the last joint of the right middle finger. I have to add as of necrotic origin the bullæ mentioned under the head of eruptions. Under atrophies come the cuticular wasting which follows some of the bullæ, and also the loss of his hair, not only from his head but from his eyebrows, which he says were very heavy, and from his eyelids.

It is worthy of note that notwithstanding this tendency to local necroses, his tissues generally repair rapidly after wound. When admitted the whole right leg below the knee was red, shiny, and swollen, while the foot was distended into a large mis-shapen boggy mass. On opening the abscess, which was most superficial on the dorsum, a large quantity of pus came away, and later on some fragments of bone, yet the incision healed rapidly and completely. I removed a tubercle from the forearm, cutting through the whole thickness of the skin, and the wound healed readily under simple wool dressing.

To complete the statement of his present condition, I may add that his heart, lungs, and alimentary system appear healthy. His urine, examined over a long period, has been normal. His deep

reflexes are normal. His temperature is, as a rule, sub-normal by about a degree Fahr. Now and again he suffers for a day from what he calls "ague." I have not been able to make such observations as would enable me to decide whether it is a symptom of his disease or a remnant of former malarial attacks, but I am inclined to the former view. Dr. Maxwell has kindly examined his eyes for me, and finds nothing abnormal in them other than some conjunctival congestion. His hearing is good, his senses of smell and taste are normal. His voice is strong, but slightly raucus. His vocal cords, seen with difficulty on account of a pharyngeal sensitiveness which, under the circumstances, is remarkable, are normal, but on the laryngeal aspect of the epiglottis there is the pearly appearance which seems to distinguish the eruption on the mucous membranes. Finally, his mental condition is distinctly good, and his intelligence of a high type considering his opportunities. Whilst he is and has always been of a cheerful disposition, I have never noticed him either unduly sanguine or gloomy.

In venturing to discuss some points in connection with the disease as seen in this man, I feel considerable timidity, both on the score of my narrow experience, limited as it is to this case, and of my slender reading on the subject, for which my ordinary duties have left me little leisure.

One of the first questions which would naturally strike one, in investigating this case is—When, where, and how did he contract this disease? The earliest sign of the disease of which I can get a trace is the chest eruption in Hong Kong, in 1866, or early in 1867. Did he then contract the disease in Hong Kong? or had he contracted the disease before he left India in 1861? This is the crucial point in his history. The rash appeared, he says, within two or three months of his arrival in Hong Kong—the restrictions on the garrison there were much greater than they had been in India; and he is positive that he never had sexual connection while in Hong Kong—of course I take such a statement merely as an expression of a probability. But the fact remains that leprosy, a disease which usually, at least, has a long incubation period,

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appeared in him two months or so after entering a leprous district; the only period during which he could have received the virus being that two months, or the period of his service in India ending five years before, or some earlier span of his foreign service. In other words, the period of incubation here was either less than three months or more than five years. I am equally at a loss as to how he received the virus. He says he does not remember to have seen a leper, or to have heard of leprosy in his wanderings, nor has he known of its existence unless from reading the Bible. Thus, so far as fear of contagion goes, he was innocent of it, and being, as I think he was, an easy-going Irishman, not particular in the selection of his company, unless according to the one criterion of its joviality, he must often have run the risk of infection from cases which were, perhaps, themselves in their early stages. This much, at least, we may say, that it is as conceivable that he should have contracted the disease from a leper's pipe or a leper's cup, as from a person so far advanced in the disease that the vaginal mucous membrane had become affected.

Turning now to the manifestations of the disease, I shall attempt to unravel what I take to be their essential order and relation. First in order comes the skin eruption, showing itself first in discrete tubercles, then in patches, appearing earliest, as in most of the exanthemata and usually in syphilis, on the trunk and face, and extending centrifugally down the limbs. In the next stage comes the extension of this superficial eruption to the mucous membranes—here, the nose, palate, pharynx and epiglottis. With this comes involvement of such thinly-covered bones as those of the nose and palate. Next comes implication of nerves, either by extension from the skin into the subcutaneous tissue where the sensory nerves lie, or deeper into the common nerve-trunks. Whether, as probably in rabies, the leprosy virus has any selective appetite (if I may so call it) for nerves, I cannot either from this case or from reading make out. Then in order from the surface inwards I suppose each tissue would tend to be affected by contiguity. The internal organs appear to suffer by metastasis or infection by the circulation; but of this the case before us presents no example, unless we discard

the story of the traumatic orchitis and look on it as a "tertiary" manifestation. But I prefer to believe that a testis which has been quiescent since 1872, and which has, I think, no present evidence of disease beyond enlargement, is, so far as the leprosy goes, a normal one.

All these phases of the disease may, and in this case, some of them do overlap one another. Before the primary skin eruption is complete the disease has involved the mucous membranes on the one hand, and the deep tissues of the limbs on the other.

One of the most interesting questions is the relation of the bullous eruption, the caries of the hand and foot bones, and the paralysis of the hands and feet, to the primary disease. Are they directly due to the specific virus acting on the spot? or are they the consequence of lesions of nerves which normally exercise in one way or another a trophic influence on the parts affected. With regard to the paralysis, motor and sensory, I think it will be granted that it is in some way the consequence of involvement in the specific eruption of the nerves above the parts paralysed—in other words, they are the evidences of a peripheral neuritis of specific character, such as is not unknown in tuberculosis, and, if I mistake not, in syphilis. In this man's right leg, in the course of the musculo-cutaneous nerve, the typical signs are found together, motor paresis, superficial dys- or anæsthesia, and considerable tenderness on deep pressure. Even in some of the anæsthetic fingers there is deep hyperæsthesia.

A direct dependence on impairment of nerves will best, I think, explain the bullæ. They arise in the upper limb only in the course of the ulnar nerve below the elbow, that is the part where it is most superficial and where the eruption is thickest, and in the lower limb mainly in the course of the external popliteal nerve. They heal readily, which they would hardly do if they were the foci of specific inflammation. I conceive them, therefore, to be analogous to the herpetic and bullous eruptions of tabes dorsalis—that is to say, that they are trophic lesions secondary to nerve implication, and are not due to the direct action of the specific virus. Whether or not their individual appearance is determined by any form of

traumatism, such as pressure, I have not been able to make out. But it is interesting in this connection to read the official Report, dated Sept. 25th, 1888, on Keanu, the Hawaiian, who was inoculated with leprosy by Dr. Arning in November, 1885. It records that he suffers from some of the signs of tubercular leprosy. In addition "the tips of forefinger and thumb of left hand are ulcerated from handling hot tin cups of tea or coffee, indicating anæsthesia." The cause of this anæsthesia is indicated in another part of the Report, thus—"Seat of inoculation, outer aspect of left forearm, upper third, shows a dark purplish scar about 1½ inch long by ½ inch wide, irregular in shape, keloid in aspect, dense and inelastic." The Report makes no mention of the condition of the muscles, which are probably intact if he can carry cups of tea about—but we can hardly escape the conviction that the ulceration of the thumb and forefinger, if directly caused by heat, is indirectly the result of involvement of some of the cutaneous branches of the median nerve in the scar. And my patient dates his first bulla to a day on which, coming out of the Royal Hospital, he had done a great deal of walking about the Dublin streets.

A more difficult problem is the nature of the caries and sinuses in the feet. Are they again specific, or are they secondary phenomena? And, if secondary, are they trophic lesions, analogous to the "perforating ulcer" of tabes, or are they of the nature of peripheral gangrene, a sort of Raynaud's disease, and correlated perhaps with the diminished superficial temperature? I believe that they too are non-specific phenomena; and I am strengthened in that opinion by the facts that they occur far away from the tubercular areas; that they arise near the nervous and circulatory periphery; and that they show a decided tendency to heal when the parts in which they occur are saved from injury and pressure. Nor would this view be upset, I submit, by the detection in the carious discharges of the bacilli supposed to be special to the disease. It would only follow the analogy of other specific diseases that here as in them the secretions due perhaps to a common-place injury should have specific characters—a fact which may be more accurately stated by saying that in leprosy as in syphilis and

probably in tuberculosis, the specific virus may be found in the discharges of non-specific lesions quite as well as in those of lesions typical of the disease.

This leads me to the question of the distribution of the specific bacilli in this case. I cannot claim any infallibility for my investigations in this direction; but I have in all doubtful cases tried to guard myself from error by mounting a considerable number of slides; and I have had my observations checked by Dr. Edmond M'Weeney, who has kindly gone over each of my preparations with me, using a higher objective than I possessed. I have used the magenta and methyl-blue method throughout.

I have found the bacilli lepræ in the following:—

Skin-tubercle.—Sections of the skin-tubercle show bacilli in large numbers wherever the new cellular infiltration occurs, and in smaller numbers outside the margins of the new cell-growth. They are found either free in the intercellular spaces, or enclosed in cells, which differ from their neighbours in being often larger, and in not taking the contrast stain and thus being colourless. The bacilli are few in number in the most superficial layer of the corium. I cannot satisfy myself that any are to be found in the epidermis. Certainly there are no bacilli-bearing cells in it.

Serum from a needle-prick in a *tubercle* gave numerous bacilli, both free and in cells.

Nose-discharge, broken up in liquor potassæ, showed bacilli in both states.

Palate-ulcer.—The pus showed bacilli in both states.

Pharynx.—Mucus scraped from back of pharynx showed cells laden with bacilli; and very few free bacilli. The cells may have been derived from the ulcer on the palate.

Sinus in foot in connection with dead bone.—The pus from the sinus showed the bacilli mostly, so far as I can make out, free; they were often of comma shape and of larger size than usual.

Dead bone from foot—very few and scattered bacilli.

Bullæ on fingers.—Out of a large number of specimens examined, I have been able to find the bacilli in a few only, and then in exceedingly small number, not more than two or three on a slide.

I have failed to find the bacilli in—

Blood from needle-prick in *apparently healthy skin*, unless in a single instance where a few appeared on the slide. Needless to say that a single such observation is open to many objections.

In *skin atrophied* after a bulla.

In *blister* induced by cantharides on *sound skin*.

In *blister* induced by cantharides on *eruption*.

In *conjunctival moisture*.

In *saliva* taken near opening of parotid duct.

In *urine*.

I have felt that it would not be right for me to try the patience of the section by going far afield from my case. I have, therefore, not entered at all upon the literature of leprosy. I claim for the case before the Academy that it exhibits some instructive analogies to the developments on the one hand of the commoner infective diseases, and on the other of some nervous ones. Lastly, seeing that the patient has been amongst us for seventeen years, it may be well to remember the possibility of our being confronted with some results of that intercourse long after this paper shall have been forgotten.

Note.—It may throw some light on the proceeding paper to give the following notes of the case taken from his case-sheet subsequent to his being exhibited to the Academy:—

Feb. 26th, 1889.—He drew my attention to the fact that his left testicle, or rather the epididymis, is swollen and painful.

Mar. 5th.—Left testis has returned to its old proportions and is no longer tender. I conclude that he must have hurt his testis in getting in or out of bed, which he does in a somewhat helpless manner.

Mar. 6th.—A large bulla on the ulnar and front aspect of each hypothenar eminence to-day.

Mar. 12th.—To-day a bulla on each ring and little finger.

Mar. 14th.—He has been complaining of malaise, rigors, and interscapular pains for the past few days. No cause discovered.

April 3rd.—He is again free from malaise and rigors.

April 13th.—He has to-day a bulla on dorsal aspect of right middle and right ring finger, and on distal phalanx of left ring finger. This is the first time for the bullæ to appear on the middle finger.

April 17th.—Two bullæ on left index finger—one on the dorsum of proximal phalanx; the other, a very small one, near the meta-carpo-phalangeal joint. This is the first time for the bullæ on the index finger.

May 31st.—The only sign of caries he has now is a sinus in the ball of the left foot, in which some dead bone can be felt. The palate ulcer has healed.

He has had no bullæ since the crop of April 17th.

FRACTURE FOLLOWED BY GANGRENE.

By W. I. WHEELER, M.D., M.Ch., F.R.C.S.;
Surgeon to, and Lecturer on Clinical and Operative Surgery, City of Dublin Hospital.

[Read in the Section of Pathology, November 30, 1889.]

The specimens which I show represent the lower end of the right humerus and the radius and the ulna of a boy, aged twelve. The history of the case is as follows:—I happened to be in the country last August, and on arriving at my destination from Dublin, about ten o'clock at night, I was asked to examine a poor lad who had fallen off a mule and hurt his arm. I found that he suffered from transverse fracture of the humerus above the condyles, and fracture of the shaft and lower end of the radius. The basilic vein was wounded. Having no special appliances, I made an angular splint from the top of a wooden box, placed the bones in apposition, and secured the splint to the limb. A small compress sufficed to arrest the hæmorrhage from the wounded vein. I directed that the dispensary doctor should be sent for, having stated previously the nature of the accident. The boy walked home, about a mile distant. I believe the dispensary medical officer visited him next day, and saw him on several occasions. About five weeks after this accident I was again in this neighbourhood, and was requested by the dispensary doctor to see the lad. He was lying in bed; his hand was absolutely black in colour; all the soft tissues on the ulnar side of the forearm had perished, and all on the radial aspect, except a small portion of the supinator radii longus muscle, about two inches from the elbow-joint. When I lifted up his hand it separated at the wrist-joint—indeed it must have been almost completely detached before I touched it. In about a fortnight after this the boy was admitted to the City of Dublin Hospital,

where I amputated his arm. On examination of the bones the following conditions presented:—Fracture of the shaft of the radius, and commencing necrosis, fracture of the lower end of radius about an inch above the articulation. Necrosed condition of the ulna and separation of about one inch of the inferior extremity of this bone, from its shaft; transverse fracture of the lower end of the humerus above the epiphyseal line. As may be observed, when this latter-described fracture took place, the periosteum appears to have peeled off in a downward direction towards the olecranon fossa; and here union has taken place between and a little superior to the edges of this fossa, corresponding to where the periosteum is separated downwards from the shaft of the humerus, or superior part of fracture: now the lower end of the shaft of the humerus came forward at the site of the fracture, and lay in the antecubital fossa. These conditions as described are very distinctly seen, and the pressure thus exerted by the lower end of upper fragment (or shaft of humerus) on the brachial artery was evidently the cause of the gangrene and necrosis. The pressure must have been very constant, and it was assisted in being maintained by means of the periosteum, which had stripped from the bone downwards, and thus tended to steady the lower end of the humerus, and also by the arm lying on a higher level than the forearm and hand. It had remained, as far as I could ascertain, in this position for a long period, as the medical gentleman referred to was not allowed to interfere by one of the parents, who was eccentric.

PERNICIOUS ANÆMIA.

By WM. ROBT. GRAVES, L.K.Q.C.P.;
Demonstrator of Anatomy, Carmichael College, Dublin.

[Read in the Section of Pathology, March 22. 1889.]

In January of last year I was consulted by a woman, aged fifty-five, who complained of shortness of breath, palpitation, numbness of hands and feet, and a drumming noise in her ears. Her face had a peculiar yellow colour; she had a hæmic murmur. I examined her blood, and found the red cells were less than half the normal number; hæmoglobin, 55 per cent. Dr. Benson kindly took her into the City of Dublin Hospital, where she remained for some weeks; subsequently she was for a week in Sir P. Dun's Hospital under Dr. Finny. In May she again came to me, and I found she had lost ground.

On the 17th June the woman sent for me, and I found her in a dying state; the red cells were less than seven hundred thousand per cubic mm. Hæmoglobin, 15 per cent.

The autopsy was made within twenty-four hours of death. The organs presented the usual appearances found in pernicious anæmia One thing, however, was peculiar—there were patches of subcutaneous emphysema on the legs, the intestines were emphysematous, and the blood in the large vessels was full of gas bubbles.

I placed all the organs removed at once in Müller's fluid, which I changed several times until it remained clear. I was much interested in and puzzled at the curious condition of the blood, and I determined to find out if possible what was the cause of the great production of bubbles in the blood.

While in Vienna last summer, working with Weigert's staining at lesions of the central nervous system, I noticed that the red cells in the blood appeared to divide themselves into two classes—

(*a*) those that stained black, and (*b*) those that refused to stain. I sought information as to why this was, and was told that some pathologists held that they were of different origin.

Thinking over this afterwards, it struck me that if I used Weigert's staining with my pernicious anæmic organs I might gain some information. On my return I stained a section of the liver from this case, and found the capillaries much distended with blood, most of the red cells refusing to stain. Dr. Scott has kindly photographed this for me as well as other sections. There were open spaces in the section here and there where the liver cells become fatty, and in the distended capillaries between these cells were masses of large bacteria; moreover, the iron pigment granules mentioned by Quincke and Kundal stained a brown black in the liver cells. I then stained all the tissues by Weigert's method, and I found everywhere in the blood-vessels and capillaries the same large bacteria, measuring 3 to 3·5 μ in length, and 1·22 μ in breadth.

These bacteria also stained in Grenacher's logwood. I could not, however, stain them with the aniline dyes. I have very little doubt that these bacteria caused the early putrefactive changes in the blood, and as I found them in the glomeruli of the kidneys and in the tubules, I think they must have existed in the blood during life. I also found them everywhere in the mucous membrane of the intestines. The intestines and stomach were atrophied, and had scarcely any mucous membrane left.

I next set to work to find out why so many red cells refused to stain in the liver capillaries, and also in the general circulation, and after many experiments I found that those cells which contained hæmoglobin stained black, and the unstained cells contained no hæmoglobin, and may be looked on as functionally inactive. I found that in the blood of a fœtus every red cell stained, that in healthy adult blood nearly all the cells stained, in the liver of this anæmic case many cells refused to stain, and in sarcoma, &c., where there was an apparent stasis of blood, all the cells refused to stain. In old blood clots in the body, where hæmotoidin crystals had formed, the red cells would not stain, but the hæmotoidin crystals

stained black. Moreover, I mixed water and blood on a slide and dried it, coating it over with celloidin, and I found that the hæmoglobin having left the stroma, the stroma refused to stain by Weigert's method, the addition of acetic acid to blood also caused the cells to remain unstained. Since then I have been fortunate enough to obtain the organs from a second case of idiopathic anæmia, and I find the blood in even a worse condition as to staining than in my first case.

Under the microscopes you will see the liver, kidney, and spleen of each case; with certain small differences you will see that they are nearly the same. In the liver you will see the pigment granules in the cells, and in the kidney you will see the cells of the convoluted tubules showing a large amount of pigment granules. In both cases the yellow marrow of the long bones has changed into red. In the spleen of No. 1 case you can see the blood-vessels blocked with bacteria, and in the kidney of the same case notice the bacteria passing into the glomerules. By the ferrocyanide of potassium and hydrochloric acid test you can see the evidences of rapid blood destruction by the quantity of iron in the liver, spleen, and kidney, in both cases. This has been shown by Ostler, Müller, Russell, and Quincke. Other livers and kidneys which I put through this process gave little or no reaction.

The replacement of yellow marrow by red has been shown to occur in other wasting conditions, and Neumann has recorded a case of profuse metrorrhagia in which the change has taken place. Everything seems to point in the direction of increased blood destruction.

It seems evident by Weigert's staining that in this disease more of the red cells refuse to stain than in health.

I have examined two cases of leucocythæmic blood stained by Weigert's method. In one the red cells were 2,600,000 in number, and the hæmoglobin only 15 per cent., yet in this case all the red cells stained faintly. In the other the cells were 2,600,000, and the hæmoglobin was 80 per cent.; in this case all the red cells stained an intense black.

By Weigert's method (acetate of copper and Weigert's logwood)

I have stained many samples of healthy and unhealthy blood. I hand round a photograph of healthy blood stained in this way. You can see that with three exceptions all the cells have stained. It seems to me to be probable that this stain, if used in testing the blood in cases of pernicious anæmia, might enable the physician to judge of the results of his treatment; for by it he could see the proportion of red cells which were functionally inactive. I cannot close without expressing an opinion which has grown on me more as I worked at these two cases. It is this. The most marked lesion is the extreme atrophy of the mucous membrane of the whole alimentary canal with the numerous hæmorrhages under the mucous membrane. Might not the red cells be robbed of their hæmoglobin at an earlier stage of their existence than is normally the case by a condition of hydræmia, or rather perhaps of hypalbuminosis of the plasma, caused by the faulty nutrition which must necessarily follow on the heels of this extreme atrophy of the whole alimentary tract; and the red cells being thus found wanting in hæmoglobin are eliminated in the ordinary way, and thus the cell destruction calls for an increased production, and you find the yellow marrow replaced by red.

I can produce a similar condition of blood on a glass slide by mixing equal quantities of water and blood. The cells in this mixture will mostly refuse to stain. I have found from the examination of hundreds of sections from these cases, stained by Weigert's method, that more cells refuse to stain (especially in the portal vessels and in the liver) than in any disease I have had the opportunity of examining.

In order to obtain reliable results in staining the blood in tissues, it is necessary to harden in Müller's fluid, cut in celloidin, and then to stain by Weigert's process. It may be found to give unequal results at first, but by patient practice all the causes of the inequality can be eliminated. It took me six months before I could get uniform and permanent results with the blood in tissues.

With the fresh blood on a slide, however, the process is quite simple.

SARCOMA OF THE CHORIOIDEA.

By ARTHUR H. BENSON, M.A., Univ. Dubl., F.R.C.S.;
Assistant-Surgeon to St. Mark's Ophthalmic Hospital;
Ophthalmic and Aural Surgeon to the City of Dublin Hospital.

[Read in the Section of Pathology, March 22, 1889.]

Sarcomatous tumours of the chorioid, though not very rare, are sufficiently scarce to make it worth while to show two cases that have recently come under my care in St. Mark's Ophthalmic Hospital:—

Case I. (S.M.O.H., 319).—Esther C., aged fifty, was admitted in August, 1888, complaining of pain and blindness of one eye.

History.—She stated that for more than a year the sight of the left eye was dim; that it gradually got worse until the sight was reduced to bare perception of light; that she had no pain in the eye until a few weeks before, when it got red and inflamed.

Condition on Admission:—Left Eye.—There is evidence of considerable iritis. The conjunctiva round the cornea is congested. The anterior chamber is shallow, almost obliterated; the pupil acts slightly to stimulation of the right retina, and no illumination of the fundus can be obtained. Tension + 1; vision, perception of light only; globe rather tender on pressure, with a constant pain. There is some external strabismus of the left eye.

The *right eye* is normal in all respects. Though the diagnosis of tumour was not fully made, it seemed probable that one existed, and as the eye was blind, painful, and glaucomatous, with no illumination obtainable, it was considered advisable to enucleate, which I did on Sept. 1st.

On opening the globe the vitreous was found quite fluid and of a dark-brown colour; the retina was completely detached, and a small tumour, with a broad base growing from the chorioid, was situated near the disc. There was no extra-ocular portion of tumour, the sclerotic and the optic nerve were apparently perfectly

normal. The globe being hardened in Müller's fluid, was handed to my friend Dr. Graves, who kindly made sections of it and confirmed our diagnosis, based on clinical and macroscopic examination. It proved to be a spindle-celled sarcoma originating in the chorioid, and containing a considerable amount of pigmentation.

Case II. (S.M.O.H., 597).—Bridget T., aged forty, was sent up by Dr. Walshe, of Kilmacthomas. She was admitted to St. Mark's Hospital on 24th January, 1889.

History.—Four years ago the sight of the right eye began to fail (after her confinement). There was no protrusion of the globe until about eight or ten months ago, and she had no pain in it, she says, till about three months ago. The pain began in one of her back teeth, and went from that to her eye.

Condition on Admission:—The *right eye* is very prominent, and its motions in all directions very limited. The lids are much stretched and enlarged, as from a long-standing cause. The cornea is transparent, anterior chamber normal, iris and pupil normal-looking, and active to stimulation of the left retina. The lens is opaque, and vision = 0. A tumour could be felt through the large soft lids that covered the protruded globe, and little or no difficulty existed in making the diagnosis of a tumour originating in the chorioid and having extended into the orbit.

The operation was done on January 26th, 1889, and the large tumour with the eye was enucleated. Some considerable difficulty was experienced in getting the tumour away unbroken, as many adhesions existed around its posterior deep parts, and the point of the scissors did, in one place, penetrate the tumour and allow the escape of a small quantity of very soft black substance. The tumour surrounded the globe as far forwards as its equator, and the optic nerve was so attenuated that its presence could hardly be recognised.

Dr. Graves has also kindly made sections of this tumour, and has found it to be a pigmented sarcoma, with some peculiarities which he will explain presently.

The late occurrence of pain in both these cases, and the long interval between the blindness and the protrusion of the eye in the second case are worth noting.

For more than three years, probably, the tumour in the second

case remained an intra-ocular one, during which time it must have grown very slowly indeed. For the last year or less it has been extra-ocular also, and in that time it has grown six or seven times as much as it did in the previous three years—that is to say, the extra-ocular rate of growth was about twenty times as fast as the intra-ocular rate.

Dr. Graves' Report on the Microscopical Examination of the Specimens.

Case I.—This is a spindle-celled pigmental sarcoma of the chorioid. Both the macroscopic and microscopic appearances are very pretty, showing well the structure from which the growth springs.

Case II. is also a pigmental chorioidal sarcoma, but it would be difficult to find two tumours of the same name that differed as widely as these two tumours do.

This is a round-celled sarcoma, and most of the cells are pigmental; while in the other case only a very few cells were pigmental.

This growth has very much thinned the chorioid, and seems to have been of slow growth at first, afterwards finding its way out of the orbit by the optic nerve, as well as by the side of the blood-vessels; it has evidently grown quickly and formed a large tumour on the outside of the eye.

From the way in which it has passed along the optic nerve, I should be very much inclined to think that perhaps this tumour may recur as multiple sarcoma in the liver, &c., and it would be interesting to watch the future progress of this case.

Many pigmental sarcomata of the chorioid do recur in this way.

TUBERCULAR TUMOUR OF THE PONS.

By J. MAGEE FINNY, M.D., Dubl.; F.K.Q.C.P.;
King's Professor of Practice of Medicine;
Clinical Physician to Sir Patrick Dun's Hospital.

[Read before the Section of Pathology, Friday, March 22, 1889.]

At the present time considerable attention is being paid to the subject of cerebral pathology, and every instance of disease which can either confirm former observations or elucidate some vexed question dealing with this subject, possesses an interest wider and greater than it could possibly attain to, if it were but an isolated pathological fact. It becomes at once another link in the strong chain of evidence upon which hang a scientific diagnosis of brain disease and a rational therapeusis. The case which I bring under the notice of the Academy is just such an instance, and it consists of a tumour which involves a very considerable portion of the pons.

It is not my intention to occupy your time by any lengthened detail of the clinical history of the case, but as perhaps its chief interest, or one of its chief interests, lies in the direct focal symptoms produced by the new growth as it implicated the various nuclei and nerve-paths situated in the pons, I shall have to crave your indulgence if I point out the information we were able to derive from the perversion or loss of function of the parts affected by the tumour, and by means of which a diagnosis of its locality could be arrived at with some approach to scientific accuracy. Indeed, to present to the Academy the morbid specimen with the bald facts of its position and nature, would be to deprive it of its real anatomico-pathological bearing and importance, and to treat the intelligence of the Academy with scant courtesy.

CASE.[a]—The patient (J. S.) aged thirty-five, a clerk by occupation, was admitted to Sir Patrick Dun's Hospital on August 27, 1888. His personal history was free from syphilitic or tubercular taint, or injury to the head. Nor was there any tuberculosis or cancer in his family history.

He states that his present illness began a week before admission by sick stomach, and in four days he noticed that the sight of the left eye became weak, and that he could not look to the left without turning his head round. At the same time internal strabismus of the left eye was established. On the following day he noticed a numbness in his right hand and then in the foot of the same side. He also could not walk without staggering, and suffered from vertigo. On inquiry of his employer it appeared that the patient was not quite himself for a fortnight or three weeks previously; that his memory had become unreliable; and the power of his right arm was noticed to be weaker.

State on Admission.—The patient was unable to walk without staggering, and the right leg was weaker than the left. There was comparative want of power in the grasp of the right hand, and in the flexor and extensor muscles of the arm and forearm. He was also unable to bring the finger of the right hand accurately into contact with various parts of his body when unguided by sight. The reflexes, superficial and deep, were normal, and there was no ankle clonus. There was partial facial paralysis on the *left* side, as the labial fold on that side was not so marked as on the right and the left corner of the mouth was a little lower than the right. Frowning and elevation of the brows could be executed much more imperfectly on the left than on the right side. The left palpebral fissure was larger than the right, and the eyelids could not be accurately closed on volition, although they are so during sleep. The tongue was protruded straight, and speech was apparently natural, although occasionally some word or syllable was slurred, though how far this was due to habit or peculiarity could not be at first ascertained.

There was marked internal strabismus on the left side, and the eye could not be brought past the middle line in attempting to look to the left. The eyes were also constantly turned to the right, and the head generally inclined to the same side.

Dr. Maxwell kindly examined the eyes on Sept. 2nd, with the following report:—"The right eye can be turned in on conver-

[a] From notes taken by Mr. L. Molloy, B.A., Clinical Clerk.

gence, but not in looking to the left. Diplopia only present when the right internal rectus and the left external rectus are called into play. There is no nystagmus. Examined ophthalmoscopically, the right fundus is normal; in the left the top of the disc is slightly hazy, and the veins are a trifle too large in proportion to the arteries. There is an absence of atrophy and hæmorrhages."

The chief complaints of the patient are as to his eyes being crooked, the existence of a numb feeling in the fingers of his right hand, and the inability to walk steadily. The bowels were very constipated, and resisted very strong purgatives. The stomach was occasionally sick, without pain or nausea.

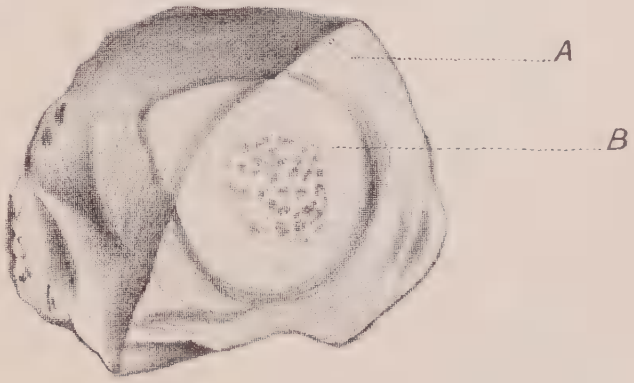
Though no complaint was made it was evident that the patient had no power, or very little, over the bladder, and he seemed to have no desire to empty it. Catheterism was, therefore, necessary twice a day for a full month after admission, when, curiously, without improvement in other respects, the bladder regained its tone, and the patient was able to pass his water without instrumental aid.

The urine was frequently examined, both on admission and subsequently, and was found free from albumen, sugar, and indigo. Its sp. gr. was 1018; chlorides were present in fair quantity. Its reaction was generally faintly acid, and sometimes neutral, or alkaline, but it was never ammoniacal or phosphatic, nor were pathological renal derivatives at any time present.

A fortnight after admission paroxysmal headache, situated in the forehead and left temple, was complained of, as well as paræsthesia of the right side of the face, nose, lips, and tongue. These parts felt numb, swollen, and subsequently icy cold, their coldness being described by the patient as "terrible;" and some short time subsequently there was complete anæsthesia of this side of the face. Speech was more indistinct and thick in utterance, and the saliva dribbled from the corner of his mouth. Profuse perspiration, which broke out on the least exertion, such as taking a drink, and mucous exudation and accumulation occurring in the bronchi, which was not removed by coughing, showed advancing disease in the pons.

These symptoms were associated with increasing muscular weakness, more especially on the right side, so that beside not being able to sit up of himself, or to maintain the sitting posture without support, the patient became unable to feed himself with his right hand, or to hold a cup between his fingers. Passing attacks of semi-coma lasting several hours now ensued, yet out of these he

LONGITUDINAL SECTION.



TRANSVERSE SECTION.

A PONS.
B TUBERCULAR TUMOUR.
C MEDULLA.
D CEREBELLUM.

DR FINNY ON TUBERCULAR TUMOUR OF THE PONS.

Lithographed by [illegible]

Falconer Dublin.

not infrequently completely rallied, and for a short time seemed a little less ill and heavy than before their occurrence.

On Sept. 26 Dr. Maxwell very kindly made a second examination of the patient's eyes. He found intense optic neuritis in each eye; the surface of the disc was not much raised, but the veins were very large and tortuous.

This change had practically occurred in little more than a fortnight; for on Sept. 2nd the right disc was apparently free of disease, and very slight alteration from normal could be detected in the left.

At this time the power of convergence of the eyes was lost, and the right eye could be turned most imperfectly and slowly to the left side, as far, and only as far, as the middle line, the movement being slow and deliberate, whereas to the right side—that to which the eyes constantly inclined—the movement was rapid and easy.

A short and temporary rise of temperature to 103° an hour after a severe rigor occurred on Sept. 27th, followed by hiccough and increasing heavy breathing and dulness of intellect.

On Oct. 3rd the ocular muscles on both sides seemed paralysed, as the lateral movements of the eyes were almost completely annihilated; and on Oct. 11th the patient died by failure of the respiration. For a day before his death the inspiration was short and jerky, and the expiration prolonged and interrupted, and the corneal, plantar, and knee reflexes, were abolished on the right side, though present on the left.

The *post mortem* examination, made the day following his death, and which was compulsorily confined to the examination of the skull, showed nothing wrong with the membranes or cerebrum, beyond a moderate increase of arachnoid fluid both outside and inside the brain. The size, prominence, and firmness of the pons to pressure with the fingers at once attracted attention, and it was readily apparent that it was the seat of some infiltration or tumour. On making a longitudinal section in the middle line this tumour, about the size of a small marble, was disclosed, and it is very accurately delineated as to size, position, and colour by my friend, Mr. Robert Lynn Heard, on this drawing (exhibited). It was chiefly on the left side of the pons, and lay about midway between its upper and lower surfaces, and occupied about the inferior half

of that organ, being accurately defined behind by the posterior limit of the pons, while its anterior and upper and lower borders insensibly blended with the nerve matter surrounding it. It occupied in a similar way the right side, but it formed but a third of the section on that side. A transverse section shows that it extended almost completely across the pons from side to side. Its colour when recent was pinkish red, and the adjoining brain matter, except near the medulla, was yellowish instead of white, as though it were in a state of fatty degeneration. It was surrounded by no marked capsule or limiting membrane, but by its pressure it appears to have completely displaced, or caused absorption of, a very considerable portion of the nerve tissue of the pons.

On examining microscopically a fresh scraping of the cut surface there were seen round cells, containing one or more nuclei, not unlike lymph cells, mixed up with a small quantity of connective tissue cells.

Dr. Purser has very kindly made a complete microscopical examination of the hardened specimen, and reported that it is an example of scrofulous tumour of the brain, remarkable alike for the extent of nervous tissue it has involved and the small amount of caseation it has undergone. The sections displayed a number of giant cells and the bacilli of tubercle.

The case is interesting, therefore, in presenting a solitary tubercular tumour of a large size in a man aged thirty-five, who had previously shown no evidence of tuberculosis.

This form of tumour—the most common of all cerebral tumours—may be either single or multiple, and when single is usually of a rounded form and is developed in the substance of the nerve-tissue. Next to the cerebellum, the pons is the most frequent seat. It is to be regretted that the restricted *post-mortem* examination precluded the possibility of verifying the existence of tubercular disease elsewhere in the body.

In summarising the above symptoms shortly after admission to hospital, the patient presented the following symptoms, which may to a large extent be taken as typical of tumour of the pons:—

(1) Incomplete paralysis of motion and sensation of the right arm and leg, with a loss of muscular sense.

(2) Paralysis of the left side of the face (alternate or crossed paralysis).

(3) Conjugated lateral deviation of the eyes to the right side, with paralysis of the left sixth nerve, and associated paralysis of the right third nerve supplying the internal rectus.

(4) Slight optic neuritis of the left eye.

(5) Unsteadiness of gait and weakness of the right leg, and a tendency to totter backwards.

(6) Paralysis of expulsive power in bladder and rectum.

(7) A fortnight later, bulbar paralysis, involving the tongue, lips, and pharynx was added, and with it a sensory paralysis of the right side of the face.

(8) And still later on, double optic neuritis of much intensity; paralysis of respiration; convulsions and coma.

Let us now consider in more detail the anatomico-pathological facts which explain the above symptoms. They are full of interest. The pathological interference commenced by the tumour involving the motor tract of the left side of the brain as it passed down from the cortical surface and the internal capsule through the pons; as the earliest symptom was a feebleness and paresis of the right arm and hand. Very soon after the leg fibres, which in their position in the pons lie external to those connected with the motion of the arm, were invaded, and the power of using the right leg was impaired.

So far, the case might have been readily taken for an ordinary one of partial or incomplete right-sided hemiplegia, and due to any focal lesion involving the upper portions of the ascending frontal and parietal convolutions, the cortical motor region, or the middle part of the internal capsule. But such a diagnosis was not tenable in the presence of facial paralysis and lagophthalmos of the side opposite to that of the paralysed arm and leg—that is, of the left side of the face. This fact—of lagophthalmos and crossed hemiplegia—at once directed attention to the pons as the seat of the pathological condition; and further, it accurately defined its situa-

tion to the lower part of the pons, for if the lesion had been in its upper part the lagophthalmos and hemiplegia would have been on the same or right side.

The ocular symptoms gave strongly confirmatory evidence in the same direction, for even before that the friends recognised the facial paralysis, that of the sixth nerve on the left side producing internal strabismus of the left eye, and diplopia on looking to the left, were early and noticeable features. This combination of paralysis of the facial with the sixth, and at the same time hemiplegia of the opposite side of the body, is very characteristic of lesion in the pons, and could hardly be produced by any single lesion other than one situated in the neighbourhood of the superior olivary body. This complexity of deranged function ceases to be a matter of surprise when the close and intimate relationship of the nuclei of these nerves is considered, and the curious way in which the root of the facial winds round the nucleus of the sixth.

The next important feature we have to deal with, and which by its co-existence with the foregoing, still further indicated the pons as the seat of disease, was the conjugated lateral deviation of the eyes to the right side. This remarkable phenomenon is one of the most common derangements of associated ocular movements which is met with in cerebral pathology. It is present in various cortical lesions—alike irritative and destructive—in the coma of apoplexy, the slightest as well as the gravest of intraventricular hæmorrhages, in lesion of the internal capsule, and in lesion of the pons. Its significance will therefore largely depend upon—(*a*) its persistent or evanescent character; (*b*) whether it be due to spasm or to paralysis; and (*c*) whether the eyes deviate to, or away from the side of the cerebral lesion.

In my case the eyes were constantly and permanently deviated to the right side—that is, to the side of the hemiplegia. Now in cortical lesions, or those of the internal capsule, the eyes are turned towards the side of the lesion, but in a paralysing lesion of the pons they are turned in the opposite direction. This, therefore, was a further indication of the seat of the lesion being in the pons, and of the left side of that organ being the part chiefly engaged.

A point of instructive interest, in connection with lesions of the pons, has been indicated by Gowers, and it was also graphically illustrated by my case. It is as follows :—"If the disease be above the nucleus of the sixth nerve—*i.e.*, at, or in the neighbourhood of, the superior olivary body—the eyes cannot be moved towards the side of the lesion beyond the middle line; but in some cases, although the associated movement is lost, yet convergence can be effected. But if the very nucleus of the sixth nerve be involved, there is complete loss of power of the external rectus, so that the eye deviates inwards, and cannot be rolled outwards, while the other eye can be moved by its internal rectus as far as the middle line, and no farther." [a]

The only other direct symptom of lesion of the pons to which I need shortly refer, is the involvement of the sensory fibres of the fifth nerve. This was strictly limited to the right side of the cheek, nose, lips, and tongue, and was not to be confounded with the more general symptoms of glosso-labio-laryngeal disease presented in the latter weeks of the patient's life. If, however, we consider the very extensive course and origin of the sensory root of the fifth, we would be rather surprised than otherwise if it were not involved by such a tumour as we have presented in this case. It is to be noted, however, that this hemi-anæsthesia occurred on the side opposite to that of the facial paralysis, and would indicate that the fibres of the fifth involved, similarly to those of the spinal nerves, had already decussated from the right to the left side.

The last symptom I will refer to is that of the double optic neuritis. This, by itself, is perhaps the least significant of focal disease of the pons; for its frequency in intra-cranial disease, and the varieties of its extra-cranial causes, deprive it largely of its diagnostic importance. But in my case its rapid and extensive formation, taken in conjunction with the other localising features to which I have given full prominence, helped not a little in coming to the diagnosis that the lesion was that of a growth in the pons causing pressure rather than one of degeneration due to arterial obstruction.

[a] Swanzy. Bowman Lecture. 1888. P. 7.

CANCER OF THE LIVER SECONDARY TO TRUE SCIRRHOSIS OF THE RECTUM.

By JOSEPH REDMOND, F.K.Q.C.P.;
Physician to the Mater Misericordiæ Hospital, and to the Fever Hospital, Cork-street.

[Read in the Section of Pathology, May 1, 1889].

William F., aged thirty-six, a cooper, was admitted into the Mater Misericordiæ Hospital, November 11, 1888. Both his parents died when he was young, but he could not say of what disease. His brothers and sisters are living as well as his own children, all of whom are healthy. He stated that he had always enjoyed very good health, and that he both worked very hard and also drank very hard. At one period of his life he was employed in a distillery, and while there consumed nothing but whisky. Recently he worked in a brewery, and drank stout, of which he consumed on an average about nine pints per day.

Fourteen weeks previously to admission to hospital he suffered from piles, and at this time lost some blood. He, however, continued at his employment for about six weeks, when he noticed that his abdomen was somewhat swollen, and about the same time he experienced a slight pain now and then in the right side. He also suffered from flatulence, constipation, and profuse sweating about head. His appetite was always good; he never at any time had nausea or vomiting. The abdominal swelling now rapidly increased to such a degree as to interfere with his stooping when working, and thus necessitated his relinquishing his employment.

On admission the patient was weak, emaciated, and very anæmic. The abdomen was tense, greatly distended, and bulged abruptly forwards below the ribs. Fluid could be detected in the peritoneal cavity. The liver was hard and enormously enlarged, extending

down to midway between the umbilicus and pubes. The surface was irregular from the presence of nodular outgrowths, some of which were slightly depressed in the centre.

While in hospital he complained a good deal of the distension of abdomen, for the relief of which a few pints of serum were removed by paracentesis, and a catheter, which was with difficulty passed up the rectum owing to the presence of a cancerous mass, permitted the escape of a large quantity of flatus.

The patient gradually grew weaker, and died on the 24th November. *Post-mortem* examination was made by Dr. M'Weeney fourteen hours after death.

On opening the abdomen about eight ounces of turbid fluid escaped from the lower angles of the horizontal incision, and almost the whole anterior aspect of the abdominal cavity was seen to be occupied by the liver, which extended completely from side to side and downwards as far as midway between the umbilicus and pubes. The intestines were mostly compressed into the left iliac region. The liver was closely adherent to the diaphragm, and enormously enlarged, weighing on removal from the body 20 lbs. 12 ozs.; shape not particularly altered, nor was the mutual relation of the lobes to one another as regards size disturbed. Scattered everywhere over the surface were very numerous, whitish-yellow nodules, some very prominent, varying in size from a pin's head to that of an orange. Some of the superficial ones were distinctly softened in the centre, and umbilicated splitting in a stellate manner during the removal of the organ. On section the same appearance was observed as on the surface.

Gall-bladder healthy, and contained about 4 ozs. of bile.

Spleen normal.

Kidneys were small and flattened on section; the pyramids were large, prominent, and pale in colour; the cortex apparently normal.

Great omentum and *mesentery* were studded with numerous nodules of uniformly small size, and almost confluent.

Small intestines.—Coils small, and almost empty.

Large intestine greatly distended, and contained a large quantity of semi-fluid fæces.

Rectum.—Just inside the anus a hard mass sprang from the mucous membrane, and almost completely encircled the gut, blocking up its lumen. It was very prominent at one point, forming a hard tumour about the size of a pigeon's egg. Above this the walls of the rectum were considerably and irregularly thickened, and about an inch higher up a tough mass of neoplasm, cutting like scirrhus, closely united the gut with the sacrum.

Some small hæmorrhoids were observed round the orifice of the anus.

Post peritoneal and *mesenteric glands* were for the most part enlarged, hard, and dark purple on section.

The *diaphragm* was thickly studded with flattened white nodules, some attaining the size of a threepenny piece. These appeared to be superficial, and not obviously penetrating the muscular substance.

Lungs.—The visceral pleura was studded here and there with small, flat, whitish masses, about one-eighth inch in diameter, which did not penetrate into the lung substance. These were especially numerous between the lobes.

Microscopic Examination.—The tumour in the rectum consisted in the main of an abundant stroma, through which were diffused small groups of cells epitheloid in type, small, and irregularly shaped. No trace of glandular structure was anywhere visible, and the structure must be pronounced a *scirrhus cancer*. The connection of the epitheloid cells of the cancer with the epithelial cells of the mucosa was not easy to trace, as the mucous membrane was destroyed, the nuclei having disappeared, though the specimen was placed in spirit immediately on removal. The epithelial and subepithelial layers had probably undergone coagulation necrosis before death.

The mass of neoplasm higher up in the wall of the rectum, connecting that viscus with the sacrum, presented a curious microscopic structure, consisting of a very abundant fibrous stroma, in which were large spaces not unlike lymph-tracks, filled with small round cells exactly like those found in the lymphatic glands.

Sections of the liver show the masses of carcinoma assuming an

encephaloid type. The cellular structure was exceedingly rich, hardly any stroma existing. The cells are small and round, almost entirely losing their epithelial character. They are closely packed in circular areas, entirely displacing the liver-cells, and each such area above a certain size is encapsuled by a thick belt of fibrous tissue. Such areas are visible in all the sections, the smallest ones containing but a few cells, while the large ones extend over whole inches of liver.

GASTRIC ULCER, WITH PERFORATION OF THE ANTERIOR AND POSTERIOR WALL OF THE STOMACH.

By JOSEPH REDMOND, F.K.Q.C.P.;
Physician to the Mater Misericordiæ Hospital, and to the Fever Hospital, Cork-street.

[Read in the Section of Pathology, May 1, 1889.]

The specimen which I have the honour to exhibit before the Pathological Section of the Royal Academy of Medicine was removed from a patient of mine who died in the Mater Misericordiæ Hospital on 7th February, 1889.

M.B., aged twenty-six; servant; unmarried; was admitted into the Mater Misericordiæ Hospital on the 4th February, 1889. She had recently been under treatment in one of the Dublin hospitals for ulcer of the stomach, but left, as her health had considerably improved. She stated that on Friday, the 1st February, about one week after her departure from hospital, when stooping over a tub washing some clothes she was seized with vomiting and severe pain in the stomach. Her abdomen became rapidly swollen, and, feeling very weak, she was helped to bed by a friend. She remained in bed for three days, but as there was no improvement in her condition she was brought to hospital on the evening of the 4th February.

On admission her condition was as follows:—She suffered from severe pain in the stomach, accompanied by constant vomiting; temperature was slightly raised; pulse small, rapid, and feeble; respiration thoracic. On examining the abdomen it was found to be greatly swollen, tender on pressure, and tympanitic all over.

5th February.—States she feels much better; vomiting ceased during the night, and pain is not so severe; pulse stronger and not so rapid.

6th February.—At about 4 o'clock in the evening she complained of severe pain in the stomach, which was relieved by treatment. About midnight she was again seized with an intense pain in the stomach, which was accompanied by a sensation as if some fluid had been poured out into the cavity of the abdomen. Symptoms of collapse now gradually developed, and the patient died at 12 noon on the 7th February, consciousness being retained till about one hour before death.

The *post-mortem* examination was made by Dr. M'Weeney, who has given me the following report of it:—

"On *post-mortem* examination death was found to be due to acute purulent peritonitis following double perforation of the wall of the stomach.

"The anterior wall of the stomach was found to be glued to the under-surface of the anterior thin margin of the liver. On examining the stomach two perforating ulcers were found, one on the anterior wall, the other on the posterior wall, situate exactly opposite one another, somewhat nearer the lesser than the greater curvature and about midway between the cardiac and pyloric ends, the opening in the anterior wall being the older, as evidenced by the presence of lymph, which glued it to the under surface of the liver. The opening in the posterior wall had no such external appearance, and the contents of the stomach had freely passed through the perforation into the lesser cavity of the peritoneum."

UNUNITED FRACTURE OF THE PATELLA.

By E. H. BENNETT, M.D., F.R.C.S.,
Professor of Surgery in the University of Dublin.

[Read in the Pathological Section, May 3, 1889.]

An example of ununited fracture of the patella, in which the fragments present unusual relations to each other and the adjoining structures of the knee-joint, has recently come into my hands.

In presenting the specimen, I have placed on the table with it two examples of similar transverse fractures united by ligament; one of these I showed to this Section of the Academy in its first session, and the other, quite similar in character, I obtained last session. These I show to contrast with this dry specimen and its cast in the recent condition, and with the dissection before me. Of the dry specimen and its cast I have the following description by Professor R. W. Smith. He writes:—

"This cast represents the knee-joint laid open in front to show the position of the fragments in an old fracture of the patella. There is an interval of four inches between them. The so-called ligamentous union usual in these fractures had not taken place; the only medium of connection seemed to be the thickened synovial membrane. The man from whose body the preparation was procured was for a short time an inmate of the South Dublin Union Hospital, shortly after his admission into which he died suddenly of apoplexy, but during his stay in the hospital he had not been observed to walk with lameness."

Taking this description, and examining the cast, it is clear, I think, that in this example of ununited fracture there was not present even the second form of fibrous union observed in many of these injuries.

To make this difference clear it is well to refer to the modes of

DR. BENNETT ON UNUNITED FRACTURE OF THE PATELLA.

A, Femur; B. Tibia; C, Skin; D. Superficial layer of superficial fascia; E, Deep layer of superficial fascia; F, Fascia lata; I, Quadriceps extensor cruris; K, Upper fragment of broken patella; L, Lower fragment of broken patella: M, Synovial sack of knee and prepatellar bursa; N, Synovial sack between upper fragment of patella and femur; O, Ligamentum patellæ reduced in length by atrophy.

Falconer Dublin.

fibrous union of fractured patella recognised by Mr. William Adams in his paper published in 1850 (Transactions of Path. Soc. of London, Vol. II.). This paper is based on the examination in London museums of thirty-one specimens, and is the foundation of our present knowledge of this subject. Of the thirty-one specimens, twelve were found to be united by fibrous tissue arranged between the fragments, which were separated by moderate intervals only, in the same manner as in the two specimens before me, which I have placed here to contrast with these ununited fractures. To this variety of fibrous union Mr. Adams gave the name, "True Ligamentous Union."

In fifteen of the examples fibrous tissue united the fragments after the manner which Mr. Adams has called "Aponeurotic." A layer of aponeurotic membrane passed either from the anterior surface of one fragment to that of the other, or in other specimens passed from the front of the upper fragment to the fractured face of the lower, which in these instances was turned forwards, or again, a similar membrane passed from the anterior surface of the upper fragment to its broken face, adhered there and passed across a greater or less gap to the corresponding margin of the lower fragment, and adhered to its fractured surface and its anterior face.

It would appear from Smith's description that the arrangement in this example of his was different, and that nothing but the synovial membrane connected the fragments. No structures entitled even to the name aponeurosis existed, and the fragments lay exposed in the synovial cavity without any fibrous structure being attached to the broken face or margins of the fractured surface.

In Mr. Adams' analysis the four specimens not referable to any of the varieties of his aponeurotic union were obscure in consequence of their being dried or otherwise defectively mounted. I cannot find recorded any case at all like this which I now exhibit. I obtained it from a man who died in Sir Patrick Dun's Hospital this year in the status epilepticus. He had been treated by me more than ten years ago for transverse fracture of the patella, which presented nothing special in its character to attract attention

except one feature. As the treatment was closing I noticed that an extreme shortening of the ligamentum patellæ had occurred, and so I took a cast of the joint to record this fact. In the cast this feature is seen, but in the specimen the ligament appears shortened to about half an inch, and there is, of course, a corresponding displacement of the lower fragment much greater than when the man left hospital. It was stated that the man had good use of the limb, and he was able to work as a carman until his increasing epilepsy killed him. Some time before his death he fell off his car and sustained fracture of the vertex extending to the base of his skull, which I may be able to exhibit at another time.

The specimen shows the knee-joint, from which only the integuments have been removed. In the site of the prepatellar bursa a synovial sac lies directly next the skin—it was rather intimately connected to the skin in the depression between the fragments. This depression is now three and a half inches in width between the fragments, and has increased greatly since the man left hospital. On opening this cavity, which at first appeared to be the prepatellar bursa, the synovial cavity of the knee-joint is found continuous with it, nor can one say where the boundary between the two exists.

Turning to the lower side first, the lower fragment is found with the ligamentum mucosum, and parts of the alar folds of membrane, and the Haversian body resting on the upper extremity of the tibia, within half an inch of the tubercle. The fragment consists evidently almost exclusively of the extra articular part of the patella at its apex. When we look for the upper fragment at the opposite side of the synovial space it does not appear to the eye at all. One can feel it, and learn by the touch that a great thickness of tissue separates it from the space.

To examine its details I have dissected, layer by layer, the tissues from the upper end of the specimen towards the knee. The superficial fascia passing down blends with the anterior wall of the synovial cavity; beneath this a great thickness of fatty tissue, representing the deep layer of the superficial fascia, is seen; beneath this again the fascia lata. These two layers pass behind

the synovial membrane, and are inserted into the margin of the articular cartilage of the trochlea of the femur. In this way the upper fragment seems absolutely excluded from any relation to the synovial cavity of the knee. Passing again to the upper extremity of the dissection, and isolating the layer formed by the quadriceps, I have traced the muscle to its attachment to the upper fragment of the patella. On raising both from the front of the femur a synovial sac is opened which forms an articulation between the bones. This sac, when searched with a probe, is found to communicate by a small opening beside the inner condyle of the femur with the knee-joint. It is difficult to explain the adhesion between the deep layer of the superficial fascia and the fascia lata with the margin of the articular cartilage of the femur, which so completely shuts off the upper fragment from the cavity of the knee, nor do I know a record of any similar specimen having been observed.

ON AN UNDESCRIBED DISLOCATION OF THE THUMB.

By WILLIAM THOMSON, M.A., F.R.C.S.;
Surgeon to the Richmond Hospital, Dublin.

[Read in the Section of Pathology, May 3, 1889.]

A CARTER applied at the Out Department of the Richmond Hospital in March last, suffering from a severe injury of the left thumb, which he had just sustained. He had been knocked down by a horse, and in trying to protect himself as he fell, had come to the ground with outstretched hand. He immediately felt great pain at the carpo-metacarpal joint of the thumb, and found that he was unable to flex the latter, except at the terminal joint. He was seen by the House Surgeon, who recognised a dislocation, but, after many trials, failed to reduce it.

The patient attended next morning and was seen by myself and my colleagues. There was great swelling about the injured parts. What was most remarkable was the attitude of the thumb. It stuck out at nearly right angles to the hand, and it could not be flexed into the palm. An examination made through the swollen soft parts enabled us to determine that the metacarpal bone had slipped from the trapesium towards the hand, and on movement the base could be felt high up in the cleft between the thumb and the metacarpal bone of the first finger. It did not project forwards or backwards, but its posterior surface was accurately on a level with that of the bone against which it lay. Turning to the articulation, which had been injured, we could feel a hollow corresponding to the normal position of the thumb-metacarpal with relation to the trapesium, and when the thumb was pushed in the plane which it occupied upwards towards the wrist, the sulcus became apparent to the eye as well.

I brought the patient into hospital, with a view to reducing the dislocation, but he refused to remain. In a few days, however, he returned, the swelling having subsided. The condition of the parts was then more readily made out, and I had an opportunity of showing the patient to Professor Bennett and others, who concurred in the diagnosis. An effort made to reduce the dislocation under ether failed.

The injury is one which has not been described in any book which I have consulted. Malgaigne, Cooper, Hamilton, and all the writers of the present day, state that two dislocations of this joint have been observed—a dislocation of the metacarpal bone forwards and another backwards. The deformity in these is, as might be expected, well marked. Malgaigne mentions that Petit states that dislocations may take place inwards and outwards also, but he does not appear to have seen or known of an instance of either injury, and he has simply mentioned the directions as those in which it is possible to have a luxation.

NOTES OF A CASE OF IMPERFORATE RECTUM; INGUINAL COLOTOMY; DEATH.

By WILLIAM BERRY, F.R.C.S.;
Hon. Surgeon, Royal Albert Edward Infirmary, Wigan.

[Read in the Pathological Section, May 3, 1889].

I was called to see this case by Dr. Foreman, who had seen the child three days after its birth, and had tried several times to get a passage by passing a large bougie into the rectum. When I saw the child it was six days old, was fairly well nourished, and of average size. Everything it took in the shape of diluted milk, and occasionally the breast milk, remained on its stomach.

I examined and found the anus patent, but on introducing my little finger I could not pass it more than two inches, the occlusion appearing to be at the commencement of the rectum. I then tried to pass a No. 10 gum elastic catheter, but without avail. We had apparently a case in a female of the fourth variety of rectal malformation (Papendorf's classification), as given by Dr. Ball in his excellent work on the "Rectum and Anus," p. 31, where he gives a good illustration. He states that "these cases are especially interesting, as they unquestionably indicate that the malformation is due to the fact that the procto-dæum has failed to meet the mesenteron, and that they cannot possibly be due to an obliteration of the rectum after complete development."—*Opus cit.*, p. 32. Operation was at this time suggested but refused.

On the eighteenth day, the child now getting restless, uneasy, and suffering from abdominal distension, but without vomiting, the parents consented to an operation.

On Feb. 6th Dr. Foreman administered chloroform, and assisted me to perform inguinal colotomy in the left groin. We thought this would be more satisfactory than trying to find the commence-

ment of the rectum, and necessitating colotomy as a last resort. There was not much difficulty in finding the colon, which was sutured to the incision and opened. A large amount of flatus and some fæcal matter, but little meconium, escaped, An absorbent pad was placed over the wound, and the child wrapped in flannel. Next day the opening looked well. The child had vomited, probably from effects of chloroform.

Feb. 8th.—The wound looked all right. Some fæces had passed, and the child seemed fairly well, the vomiting having ceased.

Feb. 9th.—The wound was red and dusky, the surrounding tissue red and gangrenous-looking. The child died in the afternoon, apparently from exhaustion.

No *post-mortem* was allowed.

This case is especially interesting, from the fact of the child living for nineteen days without any passage through its bowels. Great care had been exercised not to overload the stomach. There was very little wasting up to the time of the operation. If the operation had been done early, it is possible we should have had a favourable result.

POLYPUS OF THE GUMS.

By ARTHUR W. W. BAKER, M.D., F.R.C.S.;
Surgeon to the Dental Hospital of Ireland.

[Read in the Section of Pathology, May 31, 1889.]

These small tumours of the gums which constantly come under the notice of dental practitioners, are interesting from the fact that they present some features which resemble those found in some of the more serious tumours of the jaws. In looking up the literature of the subject, I found good descriptions of these growths in Tomes' Dental Surgery, and Salter's Dental Surgery and Pathology, also in the works of the German writers, Wedl, Arkövy, and Rothman. As none of the authors I have mentioned give sketches or diagrams of the microscopical appearances of these little tumours, I have illustrated this communication with photographs of my sections. I take this opportunity of acknowledging my indebtedness to Dr. J. Alfred Scott, who kindly photographed Fig. 2 for me; his superior skill overcoming the difficulties of a thin section lightly stained. For photograph Fig. 1, I am responsible.

The generally received ætiology of these growths appears to be as follows:—A tooth decays until one margin, at least, is level with the gum; this rugged edge irritates the gum and produces a little tumour or hypertrophy of the gum which, in a short space of time, fills the carious cavity in the tooth completely.

Now, in my opinion, this cause is not in itself sufficient to account for this growth. For while it is easy to understand how the friction of the tongue over the sharp edge of a tooth may produce an ulceration which, under certain circumstances, may become malignant, there is no such friction in the case of the gum polypus, and I do not think that the mere juxta-position of a rough edge without friction will account for the growth. I think that it is more likely to be caused by the irritating products of the decayed dentine of the tooth—in other words, by the

ptomaines formed by the micrococci and bacilli burrowing in the dentinal tubules, micro-organisms being now looked upon as the chief agents by which the phenomena of dental caries are produced. In support of this view I may mention the opinion of Wedl, who, in his "Pathologie der Zähne," states "that these hypertrophies of the gum are caused by 'die Zersetzungproducte der harten Zahnsubstanzen,' the decomposition products of the hard portions of the tooth." Also in a work published recently, entitled, "Patho-histologie der Zahnpulpa und Wurtzelhaut," Dr. Rothman, of Buda-pesth, attributes the growth of similar polypi from the tooth pulp to the effect of the decomposition products of the dentine being washed over the exposed pulp in addition to the irritating effects of the rough edge of the opening into the pulp chamber.

These gum polypi are generally about the size of a small pea, but may, under suitable circumstances, become as large as a horse-chestnut. They are of the same colour and the same degree of sensitiveness as the surrounding gum, but are much more vascular and softer—that is, bleeding and tearing with the least touch; they may consist of one or more lobules, and have generally a broad pedicle. In the mouth they are almost indistinguishable in appearance from similar hypertrophies of the pulp, and it is frequently difficult to decide with which you have to deal. I have already pointed out their relationship to carious teeth, and these polypi sometimes attain their greatest size between two teeth whose contiguous surfaces are decayed; also when the entire crown of a tooth is reduced level with the gum by caries, the roots of a molar may be widely separated by a polypus springing up between them.

I have examined between thirty and forty slides from four polypi, and have found that histologically they are essentially papillomata, consisting of branching papillæ covered with squamous epithelium, the latter in some places appearing to be rather in thicker layers than normal, and dipping down between the papillæ in such a manner as to suggest at first sight a possible proliferation of the epithelium downwards into the connective tissue. My observations on this point are at variance with the statements of Tomes and Salter, who both agree in describing the epithelial covering

of the growths in question as thin; however, in support of my observations I may quote the views of Mr. Frederick Eve, expressed in his Lectures on Cystic Tumours of the Jaws, who states that these cystic tumours are produced by an ingrowth of the epithelium of the gum, following some form of injury, irritation by decayed teeth, or long-continued inflammation which has induced an increased supply of blood to the parts.—*Brit. Med. Journal*, Jan. 6th, 1883.

Now if the irritation of decayed teeth is capable of starting such an ingrowth as Mr. Eve describes, and I think that there is every reason to rely on the accuracy of his observations, is it not extremely probable that where we have a similar cause producing polypus we would be more likely to find an excessive amount of epithelium than that it would be scanty? In fact, this proliferation of the epithelium in some of my sections is so marked as to suggest the possibility that these little tumours may be a connecting link between a papilloma and an epithelioma. However, it is only fair to add that in other sections this proliferation of the epithelium is more apparent than real, and results from the cauliflower arrangement of the branching papillæ. Some of my preparations show masses of connective tissue appearing as if imbedded in the epithelium—this is merely the result of a transverse section going through the tops of the papillæ where they are surrounded by epithelium.

Numerous large capillaries can be seen in the papillæ, which accounts for the troublesome way in which they bleed, and large masses of leucosytes may be noticed in the connective tissue indicating its rapid growth.

Our clinical experience shows that these tumours are absolutely benign, and that when they are removed together with their exciting cause—*i.e.*, the tooth extracted or the carious cavity treated, they do not recur.

EXPLANATION OF PHOTO-MICROGRAPHS.

Fig. 1 shows branching papillæ covered with a thickened layer of squamous epithelium, which shows a tendency to dip down into the connective tissue. Fig. 2 is a transverse section through the tops of branching papillæ; this is taken with a slightly lower power than Fig. 1.

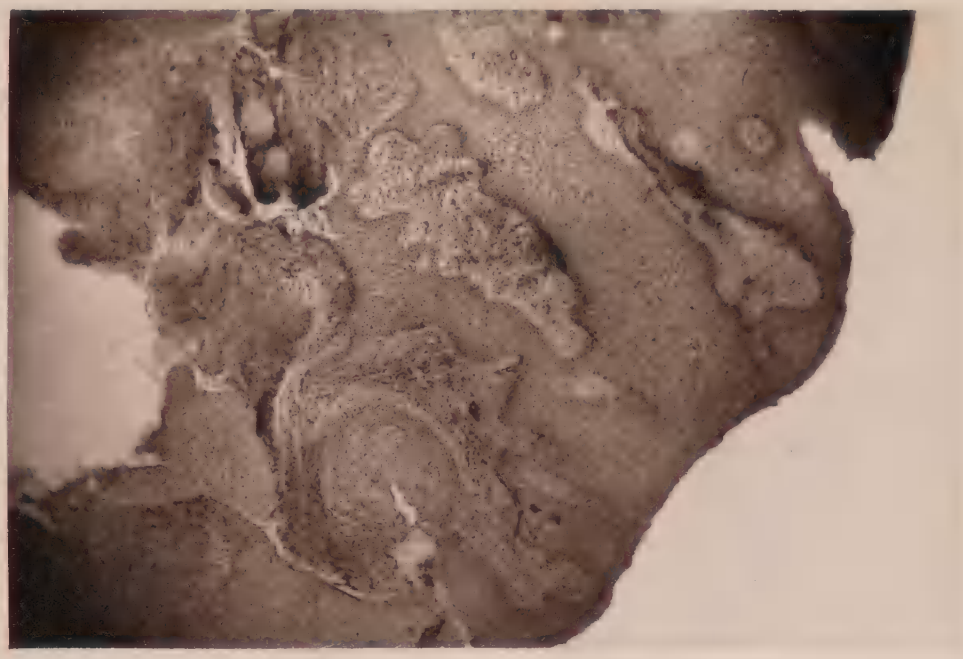


FIG. 1.

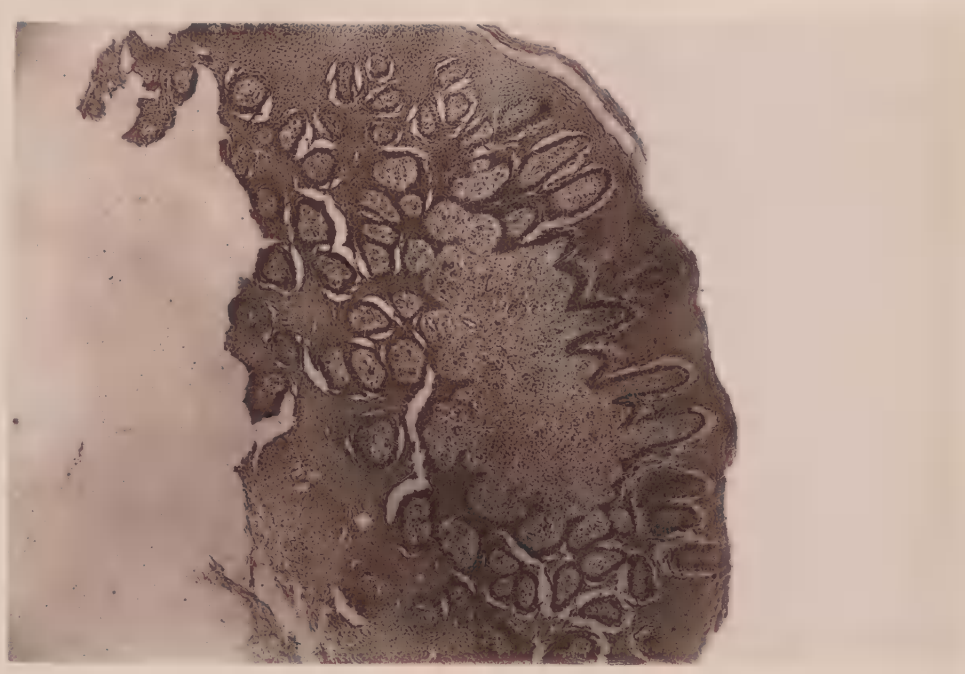


FIG. 2.

FŒTUS WITH CYSTIC KIDNEYS.

By WILLIAM BAGOT, M.B., L.M.;
Assistant Master, Rotunda Lying-in Hospital.

[Read in the Section of Pathology, May 31, 1889.]

The specimen which I wish to bring under the notice of the section this evening is a fœtus with congenital cystic kidneys. This fœtus (a female), was, after a difficult forceps delivery, born asphyxiated. During the extraction of its body I noticed that its abdomen was somewhat larger and more prominent than normal. On more careful examination I was able to palpate two symmetrical masses, one in each lumbar region, much larger and more elastic than the normal kidney of a fœtus at full term. The presence of some fluid in the abdominal cavity could also be detected. The situation, symmetry, and elasticity of the tumours made the diagnosis of cystic degeneration of the kidneys with ascites an easy one.

At the *post-mortem* (which unfortunately I was unable to obtain till decomposition had set in), I found the following:—Externally the child presented no malformation, with the exception of the enlargement of the abdomen. On opening the latter I found that it contained about 2 oz. of peritoneal fluid, a condition which was also present in all the cases examined by Dr. Bland Sutton. The peritoneum covering the kidneys was thickened, and showed signs of inflammation, bands of adhesion passing from them to the intestines, the latter being in some places adherent to each other.

The ureters, as is usual in these cases, were much thinner than normal, but pervious. The bladder and urethra were normal. The kidneys were both symmetrically enlarged and of extremely flabby consistence, but cysts were nowhere visible on their surfaces. On sections their normal structure was seen to be replaced by an

extremely fine network of minute cysts, thus resembling the variety of kidney described by Virchow as "the fine cystoid."

Dr. Earl has kindly made microscopic sections of one of these kidneys in which the tubules may be seen to have undergone an irregular dilatation, some appearing normal, others more or less dilated, till in most parts they have become replaced by large epithelial-lined spaces.

The epithelium of the tubules appears in many places to have undergone multiplication—a condition which is also present in some of the glomeruli.

The blood vessels are greatly congested, and hæmorrhages into the tissues may be seen here and there.

As regards the ætiology of this congenital cystic degeneration of the kidneys, various theories have been put forward, amongst which that of Virchow has been, I believe, the most generally accepted—namely, that it is caused by atresia of the papillæ, due to a fœtal inflammation brought about by an abundant deposit of urinary salts in tubules. Thorn believes that this condition is more frequently brought about by inflammation extending from the calices to the substance of the papillæ; while Chotinsky states that the occlusion of the tubules is, to a great extent, due to excessive multiplication of the epithelium. Some authorities, however, among whom are Shattockand Bland Sutton, hold that it is due to a primary fault of development, and that in this condition "we have really to deal with a more or less persistent mesonephros associated in greater or less degree with normal renal tissues." The appearances presented by these specimens would lead me to believe that this condition is not to be attributed so much to a primary fault of development as to some inflammatory change. The multiplication of the epithelium may, perhaps, as is stated by Chotinsky, to some extent account for the occlusion of the tubules.

SPECIMEN OF A RHINOLITH.

By E. J. M'WEENEY, M.D., M.Ch., M.A.O., R.U.I.

[Read in the Section of Pathology, March 22, 1889.]

Report on Rhinolith, by Dr. Falkiner.

In order to preserve the shape of this curious calculus I performed the whole analysis on 100 cgs. (about 15 grains), which I scraped off with a sharp knife.

The structure of the calculus was stratified, layers of dark and light colour around a cavity, which had most probably at one time contained the nucleus of the growth.

The powder examined microscopically was amorphous or in indefinite crystals.

I examined incidentally for uric acid, oxalic acid, or carbonic acid, but found no trace of them.

My analysis is nearly identical with one made by Romer, who found—

90 per cent. $Ca_3P_2O_8$.
5 per cent. $CaCO_3$.
5 per cent. organic material.

The calculus analysed by me was a cast of the position which it occupied in the nasal fossa, and I am glad to say I have been able to preserve its form consistently with an accurate analysis:—

100 centigrammes contain—

Moisture - - - -	3·2 cg.
Organic matter - - -	13·0 „
Lime (CaO) - - -	44·4 „
Phosphoric anhydride (P_2O_5) -	38·4 „

Traces of chlorine, sodium, and magnesium.

99·0 cg.

Supplemental Report by Dr. M'Weeney.

The only mention of a rhinolith, or nasal calculus, to be found on reference to the indexes to the "Proceedings of the Pathological Society of London," is in Vol. XXXV., 1884.

The exhibitor of the specimen in question is Mr. H. H. Clutton, and the stone is much smaller than that now shown, for it weighed only 65 grains, whereas the latter weighs 105. The results of the chemical analysis were precisely similar in both cases. Other curious resemblances between the two specimens are furnished by the facts that both had a central cavity, and that the patient was in both cases a young woman; the clinical symptoms were also the same—in fact, the only difference is that Mr. Clutton's specimen did not present a laminated appearance on section, whereas that now shown is most distinctly laminated.

SECTION OF STATE MEDICINE.

ISOLATION IN INFECTIOUS FEVERS.

By S. M. MACSWINEY, M.D., M.R.I.A.;
Physician to Jervis-street Hospital, &c., &c.

[Read in the Section of State Medicine, February 8, 1889.]

It would not be easy to exaggerate the importance to the community of the science of "State Medicine"—that science to the discussion of some questions arising out of whose several branches we are about to address ourselves at the meetings during the present Session of this Section of the Royal Academy of Medicine in Ireland. Public Hygiene is one of the departments of State Medicine. Under the designation of "Public Health" is comprehended the study of the physical condition of communities, and of the influences—favourable and unfavourable—which operate upon them from their surroundings. The main object of public hygiene is the prevention of ill-health, where such is possible, by the discovery of its causes, and by the inculcation and enforcing of all such hygienic rules and precautions as have been found efficacious for the stamping out, or the control of disease. A large number of maladies, and many states of impaired or deteriorated health, are now known to be preventable, chief amongst them being those included in that group which contains the infectious, or the so-called zymotic, diseases. The ravages occasionally committed by widespread epidemics of the infectious exanthemata, have been so great that the attention of the advocates of preventive medicine has been keenly directed to the discovery of means of controlling, and, if possible, eradicating these scourges of man. And regarding them—as they are now universally regarded—as due to certain

contagious particles, the inquiry at once and naturally arises, "What is the intimate nature of these contagia?" I suppose the view almost universally prevalent at the present time concerning the ætiology of this class of diseases is that they owe their existence to certain micro-organisms—organised bodies of a vegetable nature, endowed with an independent life. According to this view, infection implies the reception into the living body, from without, of some morbid agent, endowed with the power of self-propagation in the organism of the recipient. And furthermore, that these infectious germs, or morbific agents, have been contributed, directly or indirectly, by persons who themselves have been suffering from the particular disease which has been thus communicated. Some authorities, perhaps, would still dispute the correctness of the "germ theory" of disease, basing their objections to it upon—amongst other grounds—the fact of the undoubted and marked difference in susceptibility which exists in different persons in respect of these morbid poisons. Without stopping now to discuss this opinion, held by a small minority, I shall maintain that, according to our present knowledge, contagia must be regarded as living organisms, of exceedingly minute but varying size; and I shall assume that micro-organisms cause some, if not all, of the infectious diseases; that they—in a state of vital activity—possess a very remarkable power of multiplication by division—a power called forth upon their entry into the human body. Accepting this view, it necessarily follows that the duty of physicians and all concerned in preserving the public and general health is to take as effectual steps as possible to prevent the communication of such morbid poisons, by those affected by them, to others, who are, as yet, free from the disease which they are known to possess the property of communicating. For the purpose of accomplishing this object—that is, the prevention of the spread of infectious disease—a well-considered system of sanitary precautions and proceedings has been gradually elaborated, and has now reached a stage of completion which, if not perfect, is, at the least, well calculated to accomplish, when fully carried out, the more important of th ends desired by the sanitarian. Amongst the various

proceedings whose aggregate constitutes the system or code of preventive hygiene, three procedures are of paramount importance—namely (*a*) the prompt notification of the existence of the disease in the particular instance; (*b*) the destruction of its specific infective poison by a true disinfectant; and (*c*) the insisting upon the strict observance of "isolation," which, when fully enforced, will, as most of us believe, effectually prevent the spread of epidemic disease by infection.

I have selected the subject of "Isolation in the Exanthemata" for particular notice on this occasion, as, although commonly a well-understood and trite enough subject amongst the general body of the profession now, it is yet one which still requires special consideration in view of some objections which are occasionally urged against its indiscriminate adoption.

Patients, the subjects of general disease, who are too poor to afford the expense of treatment in their own homes, are usually admitted into hospital. As disease will spread from one part of a hospital where there is the simultaneous treatment of general, non-contagious disease, and cases of the different sorts of infectious fever, to the other, it soon became evident that, if the views of the nature of contagion already mentioned are to be accepted and acted on, special isolation hospitals, on the pavilion or some other system, for cases of infectious diseases should be provided where this measure of treatment could be fully carried out. Many such hospitals are now to be found in different parts of the kingdom, to which patients suffering from contagious diseases are conveyed in considerable numbers, with, it is believed, much benefit to the health and well-being of the community. The main object contemplated by the advocates of such special hospitals is to limit the spread of infectious disease by the enforcing of strict isolation.

Now, it is to the adoption of this plan of treatment of such maladies that certain objections have been urged, and the question of the advisability of isolating persons suffering from the contagious exanthemata has been raised by some authorities, who doubt or dispute the propriety of this proceeding, in whole or in part, in, at least, some instances. I shall therefore now, in the first place,

enumerate several of the statements made, and some of the arguments employed in support of this opposition to isolation, and I shall, finally, adduce certain statistical facts which are entirely subversive of some of the more serious contentions of the anti-isolationists.

I. Taking scarlatina as a type of a disease usually regarded as being pre-eminently suitable for this plan, opponents of isolation would begin by assuming that the ultimate object contemplated by advocates of such treatment was the total and permanent destruction of the malady—its removal, in fact, from the face of the earth; and they would, at once, join issue here, and deny the possibility of accomplishing this very desirable end. They would justify the position of antagonism assumed by them, by urging that whatever isolation may possibly do for other diseases, it was not to be expected that the *materies morbi* in the case of scarlatina could be exterminated by any system of isolation, contending that, from the circumstances of the case, the attempt to stamp it out would be quite impracticable, and likely to do more harm than good.

II. They would urge that isolation, strictly carried out, results—slowly but surely—in the increase, amongst the population, of the number of persons unprotected by a previous attack of the disease. That from this it would follow that there would be a yearly increasing number of persons—mainly adults—who would be unprotected, and consequently liable to be attacked, and, perhaps, to fall victims to any epidemic influence which may prevail. And that such epidemics, as a result of isolation which would cause the existence of this increased number of persons who would not be protected by a previous attack, would be found to prevail more frequently, maintaining that when the barrier of isolation once broke down, and that an epidemic did arise, there would be great danger of its becoming more extensive and virulent. So that, according to them, isolation would merely mean postponement to a more unfavourable season.

III. That it may be expected that the mortality during such an epidemic would be very great, giving, as an instance in point, the

case of the epidemic of measles which worked such havoc amongst the Fiji Islanders some years ago, when the disease arose in virgin soil. And that this mortality would probably be largely amongst adults, who would constitute the greater number of the unprotected. Moreover, that other considerations besides rate of mortality have to be taken into account when regarding the case of adults attacked by infectious diseases, such as those necessary conditions of good general health, social obligations, and business requirements which appertain to the adult, and are so seriously disturbed and interfered with by the advent of sickness, showing, in this way, that sickness, though unattended by a fatal result, was a more serious affair for adults than for the young.

IV. They would call attention to the fact that the type of infectious diseases is found to vary much in different epidemics—being now of great malignity, again extremely mild. That the rule—although undoubted exceptions must be admitted to exist—was "that they breed true," so that a mild attack would communicate a similar mild attack to others who are susceptible. Such mild attack would be followed by a low "death-rate"—a low death-rate from scarlatina would mean the addition of a certain number of persons to the general body of the protected survivors. But that isolation, if successful, would prevent this protection, and thus, in an appreciable degree, would supply increased facilities for future epidemics; so that if, by enforcing isolation in an instance where there is a likelihood of a child taking a mild attack of scarlatina, you prevent this, you are only deferring the attack to a future time under, possibly, far less favourable circumstances to the patient.

V. Again, the question is asked, "How is isolation to be carried out?" Whether in the home, or in an infectious-disease hospital? If at home, and that the unprotected children, or others, are removed to some other place, they are likely to be carriers of disease thereto, and thus spread the infection. Whilst complete isolation can, no doubt, be secured in even moderate-sized private houses, it cannot be carried out in the rooms of the poorer classes. Here removal to hospital becomes necessary. But hospitals for

the reception of cases of infectious disease may themselves, it is argued, become centres of infection. And other objections to the removal of such cases to hospital have been made—viz., that it should be done at the earliest moment, as it is dangerous to remove a patient after the first or second day of the disease, whilst before this time it may not be easy—or it may not be possible—to make the diagnosis; so that a practitioner may find himself in the unpleasant position of having sent a patient—not suffering from scarlatina—into an infectious fever hospital, with possibly very undesirable consequences to the patient or to himself. And that whilst removal to such hospital only concerns the affected person, the place whence the patient was taken may not have had any attention bestowed upon it, in the direction of sanitation, with probable results which are self-evident. Moreover, that a hospital where children attacked with infectious diseases shall be treated, cannot be easily availed of for the care of such little patients.

Now, in connection with this very question of the removal of children to hospital, Dr. Tidy, in a late report on the sanitary condition of the parish of St. Mary, Islington, made the following remarks:—"I must say that I strongly deprecate the forced removal of very young children to hospital when suffering from such a disease as scarlet fever, nor do I think this course either wise or expedient, for if we live long enough we shall all sooner or later, with few exceptions, be sure to have it, and perhaps at an age when it will be less convenient and more dangerous to suffer from it, and it may therefore be safely asserted that the rigid practice of isolation and of removal to hospital may be carried too far."

And Dr. Gairdner used these very remarkable words, when Medical Officer of Health for Glagow:—"As a general rule, the habits and traditions of the family life, even in the most poverty-stricken and degraded classes, oppose an insuperable barrier to the removal of very young children; nor, even if the consent of parents could be obtained, are the means of treatment in hospitals adapted to such cases. It is, therefore, with a feeling of complete helplessness that the Sanitary Officer sees such diseases as measles, whooping-

cough, and scarlet fever running riot in the houses of the poor; he feels that in most instances nothing can be done beyond a general instruction to open the windows, and attend, if possible, to cleanliness. In some instances he is paralysed in the face of greater emergencies; he has no power to enforce removal, even should an infected family settle in a crowded neighbourhood hitherto free from disease; he cannot disperse a school in which cases of infection have occurred, and any attempt even to give publicity would only result in their more careful concealment—even if, as has repeatedly happened within our observation, provisions are publicly sold from infected apartments. In one instance a woman with the eruption of smallpox actually on her hands was found selling sweetmeats to the children of a school in her neighbourhood. In this instance a threat, partly of exposure and partly of legal consequences, put an end to the scandal; but legal remedies can have a very limited application, and self-interest will often be much too strong for all suggestions proceeding merely on public grounds. In truth, the spread of epidemic disease among children can hardly be met otherwise than by the gradual diffusion of enlightenment, and by the improved habits which may arise from the remodelling of the dwellings of the poor over a long course of years."

Dr. Hilton Fagge ("Principles and Practice of Medicine"), after an inquiry into the physiological action of the microzymes of contagious diseases, and the duration of the immunity conferred by having once undergone an attack, says (Vol. I., pp. 29–30):—"These considerations seem to me to render very doubtful the advisability of attempting, even if it were practicable, to eradicate infectious diseases from this or any other country, and then to keep them off by a system of quarantine. Sir Thomas Watson gave to such a scheme the weight of his authority. But, sooner or later, each disease would be sure to find an entrance, and it would probably commit unheard-of ravages. I doubt whether it is even right to teach the public to look upon the maladies in question as, in a special sense, preventable." "The true method of combating infection seems to be that of which we have an illustration in the practice of vaccination." "One could not render a greater service

to humanity than by discovering how to transmit the contagion of scarlet fever through any of the lower animals in such a way that, when reintroduced into the human subject, it should convey protection without risk. I am not even sure whether it might not be well to expose children on purpose to infection from this disease, when it happens to be epidemic in a mild form."

I shall not give this paper undue proportions, by adding to it any other statements similar in tone to those which I have already brought forward. I will merely observe that I regard the several remarks which I have now adduced as constituting a very formidable array of opinion adverse to isolation, and I beg leave to submit them to the members of the Section for discussion and review. I think it will not be denied that, in the interest of the department of "Public Health," an exhaustive examination of these pronouncements is called for, whereby the contentions therein advanced may be proved to be sustainable in whole or in part, or their fallacy or unsoundness be conclusively established.

It is not my purpose on this occasion to enter upon a detailed criticism of this heavy indictment of isolation, but I shall briefly bring forward some observations which clearly show that some portions, at least, of these allegations do not rest upon solid ground.

As I have shown, it has been urged that the more isolation is carried out the greater will be the number of adults who are unprotected, who are liable to be attacked, and who may succumb to an epidemic more dangerous to them as adults. But these statements are not supported upon appeal to statistics, as the following facts will make evident:—

Dr. Sweeting (*B. M. J.*, Nov. 12, 1888) gives the following information in disproof of these assertions:—In the Western Fever Hospital, from January to November, 1887, there were 903 cases of scarlet fever admitted. Of these only 70 (less than 8 per cent.) were over 20 years of age. And again, in the four years (1882–86) 1,101 patients suffering from the same disease were admitted into the same hospital, of whom 104 (little over 9 per cent.) were adults. And as to mortality—in the above-stated two periods, of

the total mortality of 156, there were but three adults; whilst 92 per cent. of the deaths were under 10 years of age.

Again, it has been stated that the exanthemata run a severer course in adults than in children. This may be true as far as some of the exanthemata are concerned (typhus, for example); but as regards the whole class, observed facts do not support the statement—indeed the contrary would appear to be the rule. Thus, Dr. John Tatham, Medical Officer of Health for Salford, gives the statistics of the mortality in 2,500 cases of scarlatina, carefully formulated:—"Of 259 cases occurring within the first two years of life, 26 per cent. died. Of 881 cases aged from two years to five, the fatality was 14 per cent. In the next quinquennium it was 7·5; whilst among the cases occurring at all ages above 10 years, the fatality did not exceed 3·7 per cent. And, moreover, general experience would tend to show that perilous complications, not ending fatally, are more frequent in children attacked with scarlatina than in adults."

Numerous observers bear similar witness. Thus:—"During the 30 years—1851–80—the mortality from scarlet fever at all ages in England and Wales averaged 855 per 1,000,000 of the population. In infants under five, the mortality was 4·101 of those at that age; while in children between five and ten it was only 1·896 per 1,000,000 of the same age. Again, in the three London fever hospitals, of which the cases and deaths are given by Dr. Collie for the 10 years—1871–80—the percentage of deaths to cases was 26 in those under 5; 13 in those between 5 and 10; 7 in those from 10 to 15; and less than $4\frac{1}{2}$ in those between 15 and 20."—(J. S. Cameron—*Brit. Med. Jour.*, Dec. 3, 1887.)

And writing as to the influence of age in relation to scarlet fever, Thomas (Von Ziemssen's Cyclopædia) makes the following remarks:—"Age has a most decided influence on the individual predisposition. While this is very limited in the youngest children, it undoubtedly increases during the 2nd six months; is strongest from the 2nd to the 5th or 7th year, and rapidly diminishes after the 10th year; so that adults have only a slight predisposition."

At a meeting of the Epidemiological Society of London, Dr.

Whitelegge read a paper, in which he said that a detailed analysis of upwards of 6,000 notified cases showed that the liability to scarlet fever was slight in infancy, reached its maximum in the fourth or fifth year, and diminished every year afterwards. The severity of attack, however, was reached in the first two years of age, and lessened year by year throughout childhood and adolescence. In adult life there was, apparently, a slight increase again, the reality of which was open to doubt. The scarlet fever death-rate reached its maximum in the third year of life in both sexes—42 per cent. of the cases and 65 per cent. of the deaths occurred in the first five years of life, 40 and 26 per cent. respectively in the second quinquennium, and $11\frac{1}{2}$ and 5 per cent. in the 3rd. The advantage of postponing an attack was twofold, each year of age beyond the fifth diminished the susceptibility to attack, and each year of postponement lessened the average severity of attack, if it should occur; probably about two-thirds of the adult population had escaped attack altogether.

Although it is very easy for the unskilful to draw false conclusions from statistics, I think that an examination of the tables I have supplied would tend to prove that the postponing of an attack of scarlatina, instead of increasing, would largely diminish the gross mortality from the malady.

This brings to an end, gentlemen, the words which I have desired to address to you. I have adverted to one of the many questions with which "State Medicine" concerns itself. Every effort, however feeble, to advance the beneficent work of preserving the national health is of avail, and will count; and I am confident that the members of this Section who, in a special manner, devote themselves to studying the problems of public hygiene, will contribute some of the results of their labours in papers, to be read before us here. In an address on "The National Value of Public Health," by Sir James Paget, he says, in reply to the question, "What more is wanted?"—"A larger and more practical recognition of the value and happiness of good national health, a wider study and practice of all the methods of promoting it, or, at least, a more ready and liberal help to those who are striving to promote it."

THE SPREAD OF TUBERCULAR DISEASE BY CONTAGION.

By E. MACDOWEL COSGRAVE, M.D., F.K.Q.C.P.;
Physician to Whitworth Hospital, and to Cork-street Fever Hospital;
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[Read in the Section of State Medicine, April 26, 1889.]

I VENTURE to lay the following remarks before the State Medicine Section, feeling that in all probability tubercular disease will rapidly assume more and more importance in preventive medicine. It is really not many years since the majority of the medical profession shared the fatalistic views still held by the public, and indeed the very expression "preventable disease," so freely used until lately, seemed to mark out by a rigid line certain diseases which sanitary science might be hoped to lessen, separating them from all others.

Of late years the position of tubercular disease has greatly changed, and now the prophylaxis of phthisis and the hygienic treatment of its earlier stages have assumed great importance, and it is being recognised that it is contagious, and spreads by contagion. It is hardly necessary to point out the importance of tubercular diseases. In the decade 1871-80 phthisis was given as the cause of more than one-tenth of the deaths registered in Ireland—that is to say, 103,528 out of 966,745. Tubercular diseases generally cause about one-seventh of the deaths occurring in the United Kingdom.

An important advance was made early in 1882 by Koch's discovery of apparently specific bacteria in tubercle, and by his claim to have cultivated these bacteria external to the body, and apart from tubercular matter, in suitable cultivation media, and then to have developed tuberculosis in guinea pigs and rabbits by inoculating

with the organisms thus grown. It has since been generally recognised that the disease is caused by a specific morbid irritant acting on tissue of lowered vitality.

An important declaration of the opinion of medical men was elicited by the Collective Investigation Committee of the British Medical Association in 1884, in answer to a circular asking whether phthisis was contagious, and for evidence for or against. 1,078 medical men answered. Of these 673 simply wrote No, without appending any explanation. The remaining 405 reported observations. Of these 105 answered No, 39 were doubtful, and 261 answered Yes.

The full force of the affirmative evidence in these statistics is much greater than its numerical expression, for the question was asked to those whose minds were more or less prejudiced in favour of a negative answer. They had been brought up to look upon phthisis as non-contagious, and so their minds were not fairly fitted to recognise contagion. It is always hard to recognise an undescribed causation, and incidents which frequently follow the one or the other are often looked upon as cause and effect until some other interdependence is pointed out.

A good example of this fallacy is the exaggerated position the heredity of phthisis occupies in the minds of medical men. This allows observers to recognise tubercular contagion between husbands and wives, and between the sick and their attendants, but not between members of the same family. The replies received by the Collective Investigation Committee illustrate this, as out of the reported observations no less than 158 instances are given of supposed infection between husband and wife. The results of a very important series of experiments have lately been published by Dr. Cornet.[a] He examined the dust taken from the walls of hospital wards, asylums, prisons, and private houses, where phthisical patients had been. The dust was removed by a moist sponge from near the patient's bed, but from a part which could not have been soiled directly by expectoration, or contact with dirty fingers. The

[a] Die Verbreitung der Tuberkel bacillen ausserhalt des Korper, von Dr. Georg Cornet.

dust was introduced into the bodies of guinea pigs in order to test its infectiveness; 147 specimens of dust from 133 localities, and 392 guinea pigs were used in the experiments. Of the guinea pigs, 196, or exactly half, died of acute infection other than tuberculosis. Of those that did not die of more acute affections, the following percentages died of tuberculosis:—47·6 per cent. of those inoculated with hospital dust, 17·6 per cent. of those inoculated with asylum dust, and 43·6 per cent. of those inoculated with dust from private houses. Dr. Cornet considers that the expectoration is the source of contagion, that whilst moist it is comparatively harmless as of course bacilli cannot evaporate, but that when the excretion is dried and becomes pulverised, the danger of contagion is great.

This is Koch's [a] opinion also. He says—"A phthisical patient is bound, under circumstances in which these patients are at present generally found, to scatter around him a large quantity of infective material, and that in the form most likely to give rise to infection."

Fischer and Schilz' [b] experiments throw light upon this, showing as they do that tubercle bacilli may retain their virulence for forty-three days in putrefying sputum, and for 186 days in sputum dried at the ordinary temperature of the air.

The best proof of this power of contagion is the effect of ingestion of tuberculous matter by dogs, as they are not (like rabbits and guinea pigs) predisposed to tuberculosis, but rather antagonistic to it. The late M. Thuon, of Nice,[c] injected sputa of consumptives mixed with water as a spray daily for an hour during several weeks into the kennels of dogs, all of which developed pulmonary tuberculosis.

Dr. Csoker, of Vienna, records a case of tuberculosis in a dog, the master of which had died of tuberculosis some time before.[d]

A further advance was made when the possibility was recognised

[a] "Micro-Parasites in Diseases—Etiology of Tuberculosis."
[b] Ap. Koch, *loc. cit.*
[c] Ap. H. Bennet, M.D., Brit. Med. Journ., p. 387, I., 1889.
[d] Medical Press and Circular, p. 260, I., 1889.

of milk from tubercular udders causing tuberculosis. A great many observers have testified to this, and Bang, at the International Medical Congress, Copenhagen, stated that he examined the milk from twenty-seven cows suffering from tubercular mammitis, and had found the specific bacilli in the milk or sediment, and produced the disease by inoculation or ingestion of the milk or sediment.

On July 9th, 1888, Dr. G. Sims Woodhead laid the results of his researches before the Grocers' Company. Having alluded to the fact that tubercular mammitis was of extreme rarity in the human female, Dr. Woodhead showed that it was of comparatively common occurrence in the cow. Out of over 600 cows Dr. Woodhead and Prof. McFadyean examined in the Edinburgh dairies, they found 37 in which there was mammitis. In the milk of six of these they found tubercle bacilli. In one of these and subsequently in five other cases they found bacilli in enormous quantities in the udder. The new tubercular tissue was disseminated in patches of various sizes throughout a portion of the gland, the new growth invaded the lobules, so that a gradual transition from the healthy gland substance to the dense tubercular mass might be seen. In the mass itself the characteristic bacilli were present in almost inconceivable numbers. On careful examination of the gland, especially at the margin of the new growth, ulcerations into the ducts might be made out, and a small mass of tubercular granulations seen projecting into the lumen. In the granulation tissue, in the epithelial cells, and even lying free in the lumen, there were frequently numerous bacilli, and it could easily be understood how they found their way into the milk. Bacilli could also be demonstrated in epithelial cells still attached, and also in rare cases in those lying free in the apparently healthy milk ducts. Dr. Woodhead went on to state his opinion that these facts, together with the feeding experiments recorded by so many observers, went far to prove that milk was a source of tubercular infection, especially in young children.

Prof. Bang, in a paper read before the Copenhagen Medical Society on Feb. 28th, 1888, goes so far as to state that milk from cattle with pulmonary phthisis contained bacilli, even when the

udders were not in any way affected, but Dr. Woodhead's observations on the rapid and insidious manner in which the udders become affected when there is tuberculosis elsewhere, throw a doubt on this statement.

At a Congress held in Paris last year, the danger of the communication of tubercular disease from cows to human beings was fully recognised, and an important resolution carried in the following sense :—"That every means, including the compensation of owners, should be taken to bring about the general application of the principle that all meat derived from tubercular animals, whatever the gravity of the specific lesions, should be seized and totally destroyed."

Two other important resolutions were passed by the same Congress. One recommending that dairies, &c., should be inspected to ascertain that the cows are not suffering from contagious diseases (including tuberculosis) capable of being communicated to man; the other recommending that the public should be instructed in simple means of avoiding the danger incurred by using the flesh of tubercular animals, and in proper methods of disinfecting the excretæ of phthisical patients and objects contaminated by them.

The contagiousness of tubercular disease explains a great many things, the full significance of which did not before appear. For instance, it has been for long recognised that overcrowding and the rebreathing of respired air was a fruitful cause of pulmonary phthisis. In the Report of the Health of Towns Commission, 1844, and the Army Sanitary Commission, 1858, this is clearly recognised. The latter gives the following amongst other pertinent statistics :—The Foot Guards with a cubic space per man of 331 feet had phthisis mortality of 13·8 per 1,000, whilst the Horse Guards with cubic space of 572 feet, had a mortality of 7·3, or in other words, with less than double cubic space, had very little more than half the death-rate.

Contagion explains also to some extent how tubercular disease is so likely to spread in families without having to accept heredity as such an important factor as it is generally considered. Some

statistics lately published by C. Theodore Williams, M.D.,[a] are of interest in this connection.

Dealing with the improvement in tuberculosis resulting from residence in high altitudes, he gives the following table :—

	Whole no. of cases. per cent.	Cases with family predisposition. per cent.
Cured, greatly improved, ...	82·25	82·25
Deteriorated	17·02	16·12

As the improvement is just as well marked in those with a hereditary taint as in those without, it seems probable that the danger arises not from heredity as formerly understood, but from the low resistance of the weakened tissues of the offspring of consumptives, and by contagion from the consumptive to his or her offspring. A possible result of the latter may be seen in family predisposition being more common amongst women than men in the proportion of 57 to 43[b]. This may partly be accounted for by contagion, female children staying more at home and being more the subject of kissing.

Another phenomenon that the contagiousness of phthisis throws light upon is that those whose family history is clear and who show no predisposition to tuberculosis are often attacked when they have been for a long time in a debilitated condition. So in text-books, miscarriages, unfavourable confinements, over-lactation, &c., are named as predisposing causes.

From a state medicine, as well as from a medical, point of view, it is very important to recognise that tuberculosis is contagious, and that this contagion can take place in several ways—by auto-infection, by direct contagion from lower animals, by direct contagion from the human subject, and by indirect contagion through fomites.

The possibility of auto-infection is chiefly interesting from a medical point of view, but it gives a useful hint for treatment by antiseptics, destruction of excretæ, &c., and unlimited fresh air, so

[a] "Medico-Chirurgical Transactions." Vol. LXXXI., p. 325.
[b] "Quain's Dictionary," art., Phthisis.

as to reduce the danger of rebreathing the air to its lowest possible limits. It also explains some remarkable cases. I remember the late Dr. B. G. MacDowel relating the case of a young man who had advanced phthisis pulmonalis with cavities. His case was considered hopeless and, despair making him reckless, he gave up all treatment, went to the West of Ireland, spent his days shooting over the mountains, taking no precaution against cold, wet, or over-fatigue. The result was he improved and practically recovered, living for many years afterwards.

Another somewhat similar case was published two years ago by Dr. Neale;[a] the treatment, however, was carried out under medical supervision. The patient was kept warm, fresh air was freely admitted, the windows and doors being thrown open, and antiseptic inhalations were used. The result was satisfactory, the patient rapidly improving.

These seem to have been examples of desperate struggles between cells and bacteria, in which all was staked on a single throw, and in which the cells won by cutting off the bacteria from their base of supplies.

Dr. Louis Weigert[b] has lately systematised this method of treatment. Finding that the Tubercle Bacilli could bear a comparatively restricted range of temperature—37·5° C. being their optimum, 38·5° C. checking their growth, 42° C. entirely stopping their development, and 50° C. continued for a month killing them—he adopted a discontinuous sterilizing process, making his patients inhale for several hours daily air at a temperature of 150° C., or higher. As the expired air was as high as 45°, it is manifest that the temperature of the air in the alveoli must have been raised. The body temperature went up one degree. Dr. Weigert has treated a large number of cases almost throughout with favourable results, and is about to publish the clinical history of the first fifty.

The stoppage of contagion from animals is clearly in the province of State Medicine, and this has already been recognised by some States.

[a] British Medical Journal. Vol. II., p. 1068. 1886.
[b] Internat. Klin. Rundschaux. No. 51. 1888.

In France the President of the Republic last year signed a decree, providing *inter alia* that every cow found to be suffering from tuberculosis shall be isolated, and that the sanitary inspector shall be present when it is slaughtered, and make a report on the *post-mortem* appearances. The meat shall be condemned if tubercular lesions are recognised. The sale of the milk of tubercular cows is absolutely prohibited, and it can only be used for feeding animals after it is boiled. In Copenhagen the Milk Supply Company have been advised to order a fortnightly examination by a veterinary surgeon of all cows supplying milk to the company; this examination to include a most careful search for tubercular udders. In the United Kingdom we are sadly behind. The decree of the French Government might well be taken as a model. The recommendations of the French Congress as to dairies ought also to be adopted. The latter is very important in Dublin. With nearly four hundred dairy yards scattered through the city, frequently in a filthy condition, as graphically described by Dr. C. F. Moore in a paper read before this Section,[a] as sanitarians we ought not to rest satisfied until all dairy yards are removed outside the city.

The third means by which tuberculosis spreads, that is, from person to person either directly or indirectly, is also of great importance. In a paper read by the Registrar-General before this Section[b] an apparent spreading of tuberculosis from crowded centres to the surrounding districts is shown. On comparing the maps of the distribution of phthisis and of bronchitis, it is seen that the death-rate from bronchitis is high in Dublin and Belfast, but that the high rate scarcely spreads into the districts around; whilst the phthisis high death-rate does spread into the surrounding districts. One special factor in this case is probably contagion. The young go to service in towns, become reduced in resisting power, fall ill of phthisis, return home and are shut up in small houses, where they rapidly get worse, and give the disease to other members of the family. As the Registrar-General pointed out, small houses *per se* are not the cause, for in Ireland inferior house accommodation and

[a] "Transactions," Academy of Medicine, 1886.
[b] "Transactions," Academy of Medicine, 1887.

slight prevalence of phthisis are nearly parallel, not as cause and effect, but because rural life coincides with both low phthisis rate and defective housing. The prevalence of phthisis in the rural districts surrounding large cities is, therefore, a noteworthy fact.

Dr. Flick,[a] in a recent paper, gives some interesting instances of countries where phthisis was unknown until introduced by immigrants. He alludes to the way in which in cities cases of the disease occur clustered around a moving focus.

An interesting case showing the danger of contagion to those predisposed to the disease is mentioned by Dr. Williams,[b] though not with the object of illustrating this point. A young lady "with disease at the left apex, who passed one winter at Davos, lost apparently all symptoms, and nearly all signs of disease in her chest. She then married a gentleman far advanced in consumption, and went to Colorado and New Mexico, where he succumbed. During his illness he was nursed entirely by her. She returned to England in the following spring with cavities in both lungs, and with intestinal ulceration, and she died in the autumn."

Dr. Henry Bennett records the case[c] of an English officer of 27, a model of health and strength, with perfect family antecedents, no phthisis or other known taint existing. He came home from New Zealand with a young wife in an advanced stage of consumption. They occupied a small cabin, the windows of which were seldom or never opened during her lifetime. The voyage lasted four months, and she died three weeks before the ship arrived. A few weeks after his arrival in England he developed symptoms of phthisis, and died a year or two afterwards.

Dr. Bennett also says:—"My memory recalls several cases where consumption has appeared to be thus conveyed from husband to wife, from wife to husband, from mother to child, and *vice versâ*."

A case (the details of which I am personally acquainted with) still better illustrates this. A lady, strong and healthy, and of good family history, the daughter of an English medical man,

[a] "The Contagiousness of Phthisis." By Laurence F. Flick, M.D.
[b] Medico-Chirurgical "Transactions." Vol. LXXXI., p. 315.
[c] British Medical Journal, I., p. 387, 1889.

married a naval surgeon, who shortly went on a cruise. He returned a year or so afterwards, invalided, and died of phthisis in six months. His wife nursed him during his illness, and fell into ill-health. She returned to her father's house, and for nearly three years he was in constant anxiety lest she should develop phthisis. She then regained her previous good health.

I have lately observed an interesting example of contagion ending fatally. A strong, well-developed man, with good family history, worked in a telegraph instrument-room next to a consumptive. Shortly after the death of the consumptive his companion became affected with pulmonary phthisis, and in the course of two years died.

In connection with this a possible danger of health-resorts may be pointed out. Dr. Jacoby has found tubercle bacilli in the air of a hotel in Nice in July.

Against the spread of tuberculosis sanitary science can do much. The disease should be recognised as contagious, and those affected with consumption should not be allowed to imperil the health of others. G. R. MacMullen, LL.D.,[a] suggests that steamboat companies should adopt the following rules:—

1. Intending passengers should on booking produce to the booking agent a medical certificate stating that the intending passenger is in good health, or, if not so, the nature of his or her ailment.

2. The medical officer in charge of the ship should have power to remove a passenger into the ship's hospital, which, in view of such a contingency, would be properly fitted for the reception of patients.

Then precautions should be taken not to treat tubercular cases in the wards with other diseases, and the nurses in charge of such cases should, from time to time, be changed to other duties. All sputa and other excretions should be disinfected and destroyed. The bodies of animals and of human beings dying of tuberculosis should, when possible, be cremated. The non-tubercular members of the so-called "consumptive families" should be separated from those already affected. These measures, together with those

[a] Australian Medical Gazette, Nov., 1888.

recommended to check the spread of the disease from other animals, taken in conjunction with the other efforts of modern sanitation, ought to do much towards checking the spread of tuberculosis by contagion.

TUBERCULOSIS IN FOWLS.

By SIR CHARLES A. CAMERON, M.D., M.K.Q.C.P.

Past President and Professor of Hygiene and Chemistry, R.C.S.;
President of the British Public Health Medical Society.

[Read in the Section of State Medicine, April 26, 1889.]

From 3 to 4 persons per 1,000 living, die annually in Ireland from tuberculosis: the ætiology of this disease is therefore a subject the importance of which it would be difficult to over-estimate. Since the discovery of the bacillus of tubercle, the sources of the disease are the more readily traceable; that the milk of tuberculous cows was capable of communicating the disease to man was alleged and rendered probable before the discovery of the bacillus had proved the zymotic nature of phthisis. The disease may be regarded as one common to many species of animals, for the differences noticed between bacilli found in man and those detected in the ox and other animals are very slight and unimportant. The frequency of its occurrence in the animals used as food for man was best known in the case of the ox. Under the operation of the Contagious Diseases (Ireland) Act, nearly two thousand cows were slaughtered in 1888 with the view of stamping out contagious pleuro-pneumonia. The calves were, with few exceptions, free from the disease, but the healthy cows were killed because they had been on the same pastures or in the same yards with diseased cows. During the examination of the carcases of those cows at the Dublin Corporation Abattoir, no fewer than 4·9 per cent. were found to be more or less affected with tubercle. Dairy cows seem to be most frequently affected with this disease—a fact which I attribute to the insanitary conditions under which they are generally kept, and to the longer duration of their lives as compared with bullocks. I have rarely found tubercle in the pig or sheep, espe-

cially the latter, probably because those animals are slaughtered earlier in their life than the cow or even the bullock.

The transmission of tuberculosis to man from the lower animals can no longer be regarded as a *quæstio vexata;* on the other hand, it seems probable that the disease may not infrequently be conveyed from man to the lower animals—for instance, by a consumptive dairymaid to the cows she milks daily. There are three ways in which the virus of the disease may enter the system—by the lungs, the alimentary canal, or by inoculation. Dr. Tappenheimer, of Meran, in 1887, dried the sputum of a phthisical subject, and caused it to pass as dust into the atmosphere of a small chamber in which healthy dogs had been placed. After several exposures to this dust-laden atmosphere the animals were killed, and were found to be affected with tubercles.

Tappenheimer's experiments, in which the phthisical sputum was given to dogs in their food, gave negative results, but other experiments have proved that the sputa, saliva, milk, tubercular secretions, &c., taken from diseased animals, gave rise, when swallowed, to tuberculosis in calves, pigs, fowls, &c. The inoculation with tuberculous matter has produced the disease in numerous instances, and cases are on record in which human beings have contracted the disease owing to tubercular secretions coming into contact with sores or abraded surfaces on their hands.

From the evidence given in 1888 before the Departmental Committee on pleuro-pneumonia and tuberculosis in the United Kingdom, it appears that the order of liability of man and domesticated animals to contract tuberculosis is as follows:—1, man; 2, milch cows; 3, fowls; 4, rodents; 5, pigs; 6, goats; 7, sheep; 8, horses; 9, carnivora—dogs, cats, &c. The disease, however, rarely occurs amongst the domesticated carnivora.

I was one of the witnesses examined before the Committee, and I stated that I had met with many cases of tuberculosis in the domestic fowls. The affected fowls often came under my notice under somewhat peculiar circumstances. For example, in 1887 two dead hens were sent to me for the purpose of examining them for poison. The owner stated that he had lost nearly fifty fowl

within the previous three months, and that he believed their deaths must have been caused by poison, accidentally or maliciously administered. No poison was detected in the viscera of these hens, but I found their lungs extensively affected with tubercles. On making inquiries I learned that there was a consumptive patient in the house of the gentleman who had sent the hens. It was pretty clear that the sputa of the patient had been picked up by some of the hens, or that the inspissated sputa (it was in the middle of summer) had been inhaled by the animals. I have little doubt but that the disease was communicated from one hen to another. It seems that before the phthisical patient came to the house (to die, as proved) no disease had been noticed amongst the hens. The malady must have spread rapidly amongst the fowl, and have proved fatal in a very short time. In the two hens examined the tubercles were confined in one case to the lungs, but in the other the liver and spleen were greatly enlarged and much affected with tubercles. The bacilli were smaller than those which are found in man, but they appeared otherwise to be identical. The fowl were very thin, and would have brought but a poor price from the poulterer. On several other occasions fowl suspected to have been poisoned, submitted to me for examination, were found to be affected with tuberculosis, and I have one case of the kind at present in my laboratory. I have every reason to believe that fowl, and especially hens, are very often affected with tubercle. They are nearly always close to houses where sputa, recent or dessicated, are generally to be found. They often roost in the sleeping rooms of the poor; they are, therefore, peculiarly liable to contract tubercular disease from man. Does man contract this disease from them? That is a question which I am disposed to answer in the affirmative. If fowl affected with this disease be not thoroughly cooked, the bacilli which they contain may enter the body without being deprived of their vitality. A very short immersion in water at a temperature of 100 C. is, no doubt, sufficient to kill the mature bacillus, but the spores of the deadly microbe are not so easily destroyed—they may retain their vitality even when present in flesh which has been thoroughly cooked. I

frequently examine cows affected with tuberculosis, and invariably condemn their carcases regardless as to their condition otherwise and regardless as to the distribution of the tubercles. Indeed I seldom meet with tubercle in the flesh of cows except when they are very old and wasted. The safest plan is to condemn every carcase of animals intended to be used as food for man in which the bacilli of tuberculosis are detected.

In concluding this brief paper I may mention that the *Gazette Médicale* for August 15th, 1886, contains an account of the transmission of tuberculosis from fowl to a woman. Dr. de Lamallarée, who reports the case, states that the fowl acquired the disease by devouring the expectoration of a woman far advanced in phthisis, and that another woman who ate some of these fowl in an undone state speedily contracted the disease.

THE REGISTRATION OF PLUMBERS MOVEMENT IN IRELAND, VIEWED IN RELATION TO THE PUBLIC HEALTH.

By D. EDGAR FLINN, F.R.C.S.;

Examiner Royal College of Surgeons and Apothecaries Hall Conjoint Scheme; Surgeon to St. Michael's Hospital, Kingstown.

[Read in the Section of State Medicine, April 26, 1889.]

The question of the registration of plumbers has very recently come into marked prominence, and efforts are being made throughout the United Kingdom to have an authorised and reliable register kept of those engaged in plumbing work. Yet it hardly admits of doubt that until a step further be taken—viz., the compulsory registration by law of all those who follow the drainage and plumbing business, a large percentage of insanitary work will continue to be executed, and a very considerable number afraid to stand the test of examination will go on as heretofore. It is regrettable that so vital and important a branch of tradescraft, affecting as it does the public health, should have escaped the notice of our law-makers, and that no steps have been taken by the Legislature in regard to the compulsory registration of plumbers. We are all well aware that upon the care and skill bestowed in carrying out the sanitary requirements of a house depends, to a great extent, the good health of its inmates; and it cannot be overlooked that imperfect sanitary works in connection with houses results in the pernicious influences of the sewers and drains being brought to bear directly upon the occupants. Very frequently the works of house-drainage are carelessly carried out, and, as a natural consequence, untold injuries afflict the luckless tenants; delicacy and various forms of illness ensue. A death in the

household not improbably draws attention to what state the drains are in; an examination is made, and an astounding revelation is usually the result—carelessly executed plumbing work, cracks in the joinings of the drains from defective cement, holes in the leaden connections, imperfectly placed traps, soil-pipes from water closets coursing down inside the houses, and many other like defects. It is evident that, even at the present day, some time will elapse before workmen will become completely efficient in the important work of the drainage of houses; and we must hail with satisfaction the fact that a system of registration and examination of plumbers has been inaugurated in Ireland in connection with the London Guild of Plumbers. A certificate will in future be granted after examination, and, as I understand, already a large number of applications to be admitted have been received from master and operative plumbers. In London a very considerable percentage of those engaged in the plumbing craft are now duly registered after examination, and, as a consequence, sanitary works are being more skilfully and carefully carried out than heretofore.

In Dublin we stand in need of improvement in this regard; and it will be conceded that if plumbing and drainage work had been better executed formerly—for example, in the fitting-up of houses—we should hear less of typhoid fever and other preventable disorders being generated by the defective nature of pipes, drains, and closets.

A careful and conscientious man engaged in this calling has a certain responsibility, and he is aware that on the good or faulty construction of the connections and drains of a house depend, in a large measure, the health of its inmates. He has nothing to fear from a compulsory registration scheme. Yet there is a class of men who do their work in a fraudulent manner, probably because of their ignorance, or anxious to make excessive profits out of imperfectly executed work, and it is against such a class as this that it becomes indispensable that the public should have some safeguard.

The systematic inspection of house drains by the local authority,

through an expert specially trained to such work, is a question of great importance, and the plumbing connections and drainage of houses being newly erected should be examined by officials of the local authority, a certificate given that such work was satisfactorily and carefully executed, and a public register kept recording the date of the inspection, the name of the contractor, and of the individual workmen who did the drainage and its connection. If a system such as this were in existence, and recognised by law, very few contractors and plumbers would have the temerity to scamp their work. In the present day those who have charge of the inspection of drainage and plumbing work are inclined to take a very lenient view of their duty, and they not infrequently certify for work that has been imperfectly done.

The Section of Public Medicine, at the meeting of the British Medical Association at Dublin in 1887, recognising the importance of this question, unanimously passed a resolution as follows:—"That this Section, convinced of its absolute necessity in the interest of the common weal, is strongly of opinion that an organised and efficient system of registration of plumbers and of plumbing work should be put into force in all parts of the United Kingdom." A resolution couched in such definite terms as the foregoing, and with the hearty concurrence of officials high in the Local Government Board service, and the medical officers of health for such important cities as Liverpool and Manchester, affords a proof of the urgency of this question, and very recently the Duke of Westminster, the largest owner of property in London, has directed that none but certificated and registered plumbers are to be employed in new buildings on his estate, every contract on the estate providing as follows:—"That no plumber is to be employed upon the works unless he has the certificate of his efficiency from the Worshipful Company of Plumbers." Birmingham, Glasgow, Hull, Bradford, and other large towns are energetically taking up this question, and it has obtained the active intervention of influential members of the profession. Dublin, Belfast, and Cork, have also established local committees to further the movement, and in due

course of time we may confidently hope that, in the interests of the public health, this registration system will be generally adopted throughout the United Kingdom.

This movement should heartily commend itself, and deserves our earnest support, for up to a very recent date there has been little, if any, safeguard against the construction of inferior plumbing work, and the public have been at the mercy of unscrupulous builders and artisans. There are not many who have any idea of the carelessness of plumbers' work in general—of the bad junctions and imperfectly-made house connections, or of the rapidity with which pipes and drains get out of order. Good and carefully executed plumbing work is probably the most important feature, from a sanitary point of view, in the construction of a house, and this suggests the idea, which must sooner or later obtain, that plumbers should not only take charge of the actual inside work and closet arrangements, but should be made primarily responsible for the proper laying and carriage of the drains to the main sewer. This is a matter of great moment, as in a majority of cases it is usually the builder's ordinary workman or labourer who does this all-important work—a class of man who, in most instances, is lamentably ignorant of the most elementary laws of sanitation, and whose object is to get the work done anyhow, and the connection closed in from inspection as speedily as possible. It is a matter for regret that so many urban sanitary authorities, having laid down, at a great expense, an extensive system of main sewers, do not follow the good work up by enforcing the owners of house property to see that their premises are properly and scientifically drained and connected with the main sewer. Some of the best sewage schemes have been brought into disrepute by the neglect of this precaution, owners and occupiers of houses being allowed to make their connections with the main sewer whenever it pleased them, and employing the most ignorant of workmen and the cheapest materials.

This registration of plumbers scheme cannot fail to have a beneficial influence on the future of sanitary work in this country,

and it is a pleasing thing to learn that the trade has taken the question up warmly, and that arrangements are being made for the examination of a number of candidates anxious to possess themselves of the certificate. A greater amount of responsibility will henceforward be attached to the master plumber, who will be made amenable and more liable for the faults of his workmen. This in itself will be a great advantage to the public, and a most necessary point will thus have been achieved.

SECTION OF ANATOMY AND PHYSIOLOGY.

ON THE NERVE SUPPLY OF THE STERNALIS IN AN ANENCEPHALOUS FŒTUS.

By A. BIRMINGHAM, M.B.;
Professor of Anatomy, Catholic University School.

[Read in the Section of Anatomy and Physiology, Jan. 4, 1889.]

This is a subject of considerable interest at present, owing to the fact that an intercostal supply has been described for the muscle, both in addition to and without the anterior thoracic supply described by Professor Cunningham. In this case the sternalis was very large, and the central part of the great pectoral was absent on both sides. The usual nerve supply (from the external anterior thoracic nerve) was easily found, but in addition there was a twig of the fourth intercostal nerve, which pierced the intercostal muscles, and was distributed to the sternalis, entering its deep surface, and ramifying in its substance. The intercostal supply described was found only on the left side. At this stage of the dissection the specimen was shown to Professor Cunningham, who was also of opinion that this was an intercostal supply for the muscle in question. To remove all doubt on the matter, Dr. Brooks kindly examined the nerve and bit of muscle by his ascetic acid method, teasing out the specimen under the microscope. His examination changed the opinion formed about the nerve supply; it showed that the twig from the intercostal, which, as far as could be judged by the naked eye or a hand glass, was a nerve, contained no true nerve fibres, but was simply connective tissue. The case was brought forward to show how difficult it is, by ordinary naked eye examination, to recognise small nerves in such cases as this, and how easily mistakes may be made.

REPORT OF THE COMMITTEE OF INVESTIGATION IN THE ANATOMICAL DEPARTMENT OF TRINITY COLLEGE. SESSION 1888–1889.

By D. J. CUNNINGHAM, M.D. (Edin. and Dubl.);
Professor of Anatomy.

[Read in the Section of Anatomy and Physiology, March 15, 1889.]

The excellent results which have been obtained by the method of collective investigation applied to elucidation of disease led me, some years ago, to believe that much might be gained by the application of a similar method to anatomical investigation. To test the feasibility of such a project, a small committee was organised at the beginning of the present Session (1888–89) in the Anatomical Department of Trinity College. The results have proved so satisfactory that I have now no hesitation in proposing that under the auspices of this Section of the Academy the scheme should be extended so as to take in all the Anatomy Schools in Ireland. Prof. Macalister, of Cambridge, has already initiated a similar scheme. In the programme of last week's meeting of the Anatomical Society in London, it was announced that he would propose a scheme of collective work. There is no one better qualified to deal with this matter or to take the lead in the work when the details are finally agreed upon. Prof. Macalister's own work on the statistics of anatomy are unsurpassed in this country, and I am satisfied that the scheme which he will propound will be in all respects satisfactory. Further, I think I may answer for the majority of the Irish anatomists, and promise that when the time comes they will be ready and willing to bear their share of the work. Until, however, the general scheme proposed by Professor Macalister is made known, and the preliminary details arranged, I consider that we in Ireland might make a trial of the project. Indeed I am not with-

out hope that the reading of this Report may lead to such a result.

The Committee in Trinity College was organised in this way:— Having drawn up a list of a number of questions in human anatomy, which I believed might very well be investigated by the senior students without making too great a demand upon their time, I invited the members of the class to take part in the work. Instructions were given that each subject that came into the room was to be examined with reference to the particular points under investigation. In this manner we obtained six energetic workers, all of whom (with one exception) have handed in reports.

Dr. H. St. John Brooks' *Report on the Relations of the Pleural Sacs to the Chest Wall.*

Dr. Brooks, who kindly undertook a share in the work, has made a number of careful observations on the relation of the two pleural sacs to the anterior wall of the thorax. I have frequently been struck with the variations which are exhibited in these relations, and suggested to Dr. Brooks that he should test the accuracy of the very full and excellent accounts which are given by Luschka and several other anatomists. This he has done in his usual careful manner, but as his investigations are not yet completed we have thought it better to hold back his report until the publication of the next volume of the Transactions.

Mr. O. L. Robinson's *Report on an Occasional Eighth True Rib in Man.*

In the number of *Nature* which appeared on the first of November, 1888, there is a short notice to the effect that at the meeting of the Anatomical Society, during the Session of the Medical Congress in Washington, Dr. Lamb, of the United States Army Medical Museum, referred to the occasional elongation and direct articulation of the eighth costal cartilage with the sternum. At the same time he reproached anatomists with the small amount of attention which had been paid to this anomaly. He remarks that he has made a thorough search in anatomical literature for references to

this peculiarity. According to *Nature*, he is represented to have stated "that in the English books there is only a single incidental reference to it, and in that case the author does not say that he has ever seen a specimen. In German text-books there are two references, one of them being the one already mentioned by the English authority, &c., &c." This reproach is altogether undeserved, because there is not a single writer of importance in Germany who does not mention the fact that occasionally the eighth rib is joined directly to the sternum by its cartilage. References to the condition are to be found in the works of Humphry, Krause, Henle, Aeby, Hyrtl, Gegenbaur, Meckel, Oehl, of Pavia, &c., &c.[a]

The occasional occurrence therefore of an eighth true rib in man is an anomaly which is well known to anatomists. At the same time, however, it must be admitted that statistics as to its frequency of occurrence, and several other points in regard to it, are much wanted. I was, therefore, induced to ask Mr. O. L. Robinson, Assistant-Demonstrator of Anatomy, to undertake this subject. The results which he has obtained are extremely interesting and important.

In all Mr. Robinson has examined seventy subjects. Of these *fifty-two* were brought into the room for dissection; *five* were skeletons in the Trinity College Bone-room;[b] *eight* were fœtuses from the stores of the Anatomical Department (two being anencephalous), and *five* were dried lecture specimens. With regard to sex, forty-two were females and twenty-one males, whilst in seven the sex was not ascertained. But as the anomaly did not occur in any of the last, the determination of the sex was a matter of no special importance.

It is certainly surprising that in these seventy subjects Mr. Robinson discovered an eighth true rib no less than fourteen times.

[a] In a letter which was published in *Nature* (Jan. 10, 1889), I called attention to this misrepresentation, and a month or so later Professor Anderson, of Galway, did the same in the Anatomischer Anzeiger (Feb. 12, 1889).

[b] With reference to these skeletons, it is a curious coincidence that the eighth true rib is present in three.

This gives a percentage of 20 and stands in marked contrast to the statistics of Hyrtl,[a] who examined thirty subjects and only observed the anomaly in three.

In the fourteen cases observed by Mr. Robinson, the attachment of the eighth rib-cartilage to the sternum was effected very differently in the different examples—(1) In some of the cases it articulated directly with the lower end of the last piece of the gladiolus; (2) in others it simply rested on the anterior surface of the ensiform cartilage, and was connected to this by some ligamentous tissue; (3) in those cases where it was bilateral the two cartilages, in the majority of instances, articulated with each other in front of the ensiform cartilage, and were bound to this and also to the gladiolus by ligaments; (4) in one case a narrow piece of separate cartilage, attached by fibrous tissue, on the one hand to the tip of the eighth rib-cartilage, and on the other to the sternum, represented the connection. This last instance resembles somewhat a case recorded by Oehl,[b] of Pavia.

The connection of the eighth rib-cartilage to the sternum was more frequently unilateral than bilateral. The proportion was as follows :—

Unilateral . . .	9 cases
Bilateral . . .	5 cases.

Four of the cases in which the connection was present on both sides were in males, whilst the remaining instance occurred in a female anencephalous fœtus.

Turning, in the next place, to the analysis of the nine instances in which the condition was unilateral, it is certainly a matter of much interest to note that eight of these occurred on the right side and only one on the left side.

With regard to the sex, seven of the observed cases of eighth true rib were in males and seven in females. But from this it must not be inferred that the condition occurs equally in the two sexes. If this were the case the proportion would have been 4·7

[c] *Vide* Henle's Knochenlehre, p. 61.

[a] Sulla presenza di un' articolazione costozifoidea. Wiener Sitzungsber. Band. XXXII.

in males and 9·3 in females, as the number of females examined was twice as great as that of males (42 of the former and 21 of the latter). We may conclude, therefore, that the anomaly occurs more frequently in the male. This hardly bears out the observations of Hyrtl, who has seen an eighth true rib three times, and always in the female. From this he was led to believe that the condition was produced by the compression of the chest, which results from the wearing of tight corsets—a conclusion which, even apart from the statistics supplied by Mr. Robinson, is manifestly absurd.

But the preference of the anomaly for the right side is a matter of very special interest. In only one of the unilateral cases as we have noted was it seen on the left side. It is possible that the true explanation of this is that the anomaly is connected with the more frequent use of the right upper limb, or, in other words, with right-handedness. An additional rib attached to the sternum would undoubtedly contribute, in a slight degree, to increase the stability of the basal support of the right upper limb. This view is still further supported by the fact that the extension of the eighth costal cartilage to the sternum occurs more frequently in the male, who makes greater demands upon the right upper limb than the female.

That the occasional eighth true rib in man is a reversion to type is rendered likely by the fact that it is the normal condition in the ape. It is true, however, that this condition is not universal amongst the anthropoids. In the chimpanzee there are eight true ribs on each side, but not so in the orang or the gibbon. The gibbon has its eighth costal cartilage prolonged forwards so as merely to touch the ensiform cartilage without articulating with it, whereas in the orang the eighth costal cartilage is arranged as in man.

Dr. Lamb, of the United States Army Medical Museum, has observed twelve cases of an eighth true rib in man. In all of these, with one exception, the anomaly occurred in negroes, the exceptional case occurred in an Indian.[a] Some observers have thought that the condition is especially frequent in the black races,

[a] Nature, Nov. 1st, 1888, p. 17.

but according to Luschka it is not more common in them than in other races of mankind.[a]

Mr. R. L. Heard's *Report.*—(*a.*) *On Cervical Ribs ; (b.) On the Achselbogen Muscle.*

Mr. R. L. Heard, Assistant Demonstrator of Anatomy, undertook to examine the various subjects which were brought into the dissecting-room for the cervical rib. It is my belief that this occurs more frequently than is generally supposed, but that in many cases it is so small that it escapes notice unless it is specially looked for. Although Mr. Heard worked in a most efficient manner, and examined both sides of more than fifty subjects, he did not detect a single example of the anomaly. He was so fortunate, however, as to find a condition of considerable rarity, although not of such interest from a morphological point of view. I refer to a great reduction in the size of the first rib. At first sight the specimen presented very much the appearance of a cervical rib, and this impression was strengthened by the similarity which the second rib presented to an ordinary first rib. Thus the second rib was flattened, its surfaces were directed upwards and downwards, the upper surface showed the vascular impressions for the subclavian vessels and its cartilage articulated with the upper part of the lateral border of the manubrium immediately below the clavicular facet. The first rib, on the other hand, was stunted and narrow, and its costal cartilage was replaced by a fibrous band which connected it with the upper corner of the manubrium sterni. This anomaly has been noticed by Struthers,[a] Cruveilhier,[a] Luschka,[b] and some others. Several different varieties of the condition have been described. The first rib may be reduced to a length not exceeding one inch; the fibrous band which maintains the continuity of the bone may be attached to a stunted costal cartilage, to the sternum, as in Mr. Heard's case, or to the first rib. Sometimes the first rib beyond the tubercle loses its identity by

[a] Luschka's Anatomie der Brust des Menschen, 1863, p. 119.

[b] *Vide* Henle, Knochenlehre, p. 73 ; also Luschka's Anatomie, der Brust, p. 118.

becoming completely fused by bony union with the second rib—the two bones forming a broad flat plate. While lately examining the skeletons of some of the low races in the Museum of the Royal College of Surgeons of England, I met with a very beautiful example of this last form of the anomaly in a male Australian (No. 1,045 in the Catalogue).

Mr. R. L. Heard also took careful notes and made accurate drawings of all the cases of achselbogen muscle which appeared in the dissecting-room during the session. The anomaly was discovered in five female subjects, and as it was present on both sides of three of these, Mr. Heard had the opportunity of examining 8 muscles. The origin presented by these muscles was very different in different cases:—

(*a.*) In three cases it arose from the superficial surface of the tendon of the latissimus dorsi.

(*b.*) In other three instances some fibres arose from the superficial surface of the latissimus dorsi, whilst others were directly continuous with those which formed the anterior border of that muscle.

(*c.*) In the remaining two cases, both of which occurred in the same subject, the achselbogen muscle was directly continuous with the border of the latissimus dorsi; not the slightest indication of any demarcation could be detected. In both of these instances the anomalous muscle formed a complete muscular floor for the axilla.

Nothing remarkable was noticed in connection with the insertion of these muscles except in one case. In this a long tendinous dorsi-epitrochlearis was present, and it took origin, not from the latissimus dorsi, but from the tendon of the achselbogen.

In those cases in which the nerve of supply was found it came from the internal anterior thoracic.

The evidence as to the nature of the achselbogen muscle, which is afforded by the attachments of the muscles observed by Mr. Heard, is somewhat conflicting. On the one hand, some of the cases appear to support Professor Birmingham's view, that the achselbogen is a superficial muscle and is to be referred to the panniculus carnosus; whilst, on the other hand, others of the cases

distinctly point to an opposite conclusion, and would lead one to infer that it belongs to a deeper stratum, or, in other words, that it is a part of the layer which in this region is represented by the pectoralis major and the latissimus dorsi. Very suggestive is the case in which the dorsi-epitrochlearis muscle is associated with the achselbogen. For my own part I am inclined to believe that in the region of the axilla there is not the same sharp, well-defined sub-division between the panniculus and the deeper muscular stratum that exists elsewhere.

Mr. R. R. Brunskill's *Report on the Musculus Sternalis.*

During the winter session of 1883 I traced for the first time the nerve-supply of the musculus sternalis, and showed that it came from the anterior thoracic nerves. Since then the same nerve has been traced by different observers no less than thirty-six times[a] (viz., Shepherd 10, Wallace 1, Lamont 6, Dwight 2, and myself 17). It was with some astonishment, therefore, that I read an article in the *Anatomischer Anzeiger* (May, 1888, Nos. 11 and 12), in which Professor Bardeleben asserts that in eleven cases of this anomaly he has traced the nerve-supply from certain of the intercostal nerves. Such a supply I have always looked for in vain, but that it may occur associated with the true nerve-supply from the anterior thoracic nerves is proved by Professor Shepherd, of Montreal, who on two occasions in anencephalous fœtuses has observed a twig from one of the intercostal nerves entering the sternalis muscle in addition to the invariable branch from the thoracics. That the intercostal supply should in certain cases *entirely* replace the thoracic supply I have some difficulty in believing. Indeed, until the appearance of Professor Bardeleben's paper I had thought the point settled and at an end. The publication of this paper, however, rendered it necessary to reopen the question, and I asked Mr. R. R. Brunskill to undertake the work. He examined sixty sub-

[a] For references v. article on "The Musculus Sternalis," by D. J. Cunningham, Journal of Anatomy and Physiology, Vol. XXII., p. 391 ; also article on "The Musculus Sternalis and its Nerve of Supply," by Prof. Shepherd, in the same journal, Vol. XXIII., p. 303.

jects consecutively, and in four of these (in two females and in two males) he met with the musculus sternalis. In one male and in one female the muscle was present on both sides, and thus Mr. Brunskill had the opportunity of examining six muscles. The nerve-relations were worked out with the greatest care, and with the result that in no instance was any nerve-filament from the intercostal nerves traced into the musculus sternalis. In the majority of cases the intercostal nerves, with the exception of the first and perhaps the second, were strongly recurrent. They pierced the pectoralis major under cover of the sternalis muscle, and then hooked round the inner margin of the latter to reach the surface. The significance of this arrangement I have discussed in my paper upon the musculus sternalis published in the *Journal of Anatomy and Physiology* (Vol. XXII.). The first and the second intercostal nerves either pierced the sternalis, or made their appearance on the outer side of it.

In all the six cases Mr. Brunskill was able to trace the nerve-supply of the anomalous muscle. It came from one or other of the anterior thoracic nerves, but in the majority of instances from the external anterior thoracic. This record, therefore, brings up the number of cases in which the nerve-supply of the sternalis has been traced from the anterior thoracic nerves to forty-two. It is interesting to note that in the female subject in which the muscle was double, Mr. Brunskill found a triple nerve-supply to the left sternalis and a double nerve-supply to the right. On the left side two of the nerves came from the external anterior thoracic and one from the internal anterior thoracic; on the right side the two nerves arose by a short common trunk from the external anterior thoracic nerve. So far as I am aware, no case similar to this has been described before.

It is needless to enter again into the question of the homologies presented by the musculus sternalis. The cases observed by Mr. Brunskill serve still further to confirm the views which I expressed in the article already referred to. As bearing to a certain extent upon this matter, I may mention that Mr. Brunskill discovered a very interesting muscular anomaly in the pectoral region of a male

subject. It consisted in a narrow band of muscular fibres, which arose from the junction between the manubrium and gladiolus of the sternum, and extended transversely outwards on the surface of the pectoralis major. It was inserted into the deep fascia covering the anterior fold of the axilla, and received its nerve of supply from the external anterior thoracic. The interest of this anomaly is centred in the fact that it undoubtedly belongs to the sternalis group. In fact I look upon it as a sternalis in which the rotation of the pectoral fibres which compose it has been arrested in its downward movement. It is to be regarded then as a valuable addition to our evidence in connection with the sternalis question.

In bringing this report to a close I may say that my anticipations in regard to the good resulting from the formation of such a committee in an anatomical department have been more than realised. The amount of work required by the different students engaged did not interfere with their regular studies, and they acquired some knowledge of the manner in which investigations should be pursued, and of the accuracy and close observation which are essential in all forms of research. But the advantages accruing were not confined to the members of the committee; the great majority of the students attending the practical class were interested in the work and lent a helping hand towards its furtherance. Consequently they were brought under whatever influence for good there was.

ON THE RELATIONS OF THE PLEURA TO THE STERNUM AND COSTAL CARTILAGES.

By Dr. St. JOHN BROOKS,
Demonstrator of Anatomy, Trinity College, Dublin.

[Read in the Section of Anatomy and Physiology, March 15, 1889.]

In consequence of the prevalence of pulmonary and pleural diseases in dissecting-room subjects, I have only succeeded in examining seven bodies in which the pleural sacs were perfectly healthy. As my results are somewhat at variance with the generally received description, I wish to explain that I should not have brought forward any remarks founded on the observation of such a small number of cases, were it not that I believe that the method employed is very precise. The pleura has been most carefully studied and described by Luschka and others, and the statistics of Bochdelek are founded on the examination of a large number of subjects (viz., 56 adults and 81 children). The method employed by me was as follows:—A tracing was taken of the sternum and ribs after they had been defined by the removal of the intercostal muscles. The sternum was then divided by the saw in such a manner as to remove the portion opposite the articulations of the third rib on the left and the third and fourth ribs on the right side. The piece of bone was carefully removed so as to leave the posterior layer of periosteum untouched. This tough periosteum maintained the pleura undisturbed in its natural position, and the exact point of reflection could be observed and recorded on the tracing. The upper part of the body of the sternum and the lower part of the manubrium were then removed with the same precautions, and, lastly, the lower part of the body, including the extreme base of the ensiform, was treated in a similar way.

It is stated by Luschka that the line of reflection of the left

pleura normally diverges from the sternum opposite the upper border of the cartilage of the fifth rib, and from that point recedes further from the sternum as it passes downwards. At the level of the sternal end of the fifth rib it is 1·5, and at the seventh rib 3·5 centimetres distant from the left border of the sternum.

I found in 4 out of my 7 cases that the reflection was entirely under cover of the sternum, while in another it was quite close to the left border of that bone. In all my cases I have found that the right pleura ran down either in contact with or very close to the left pleura as far as the ensiform, and, in consequence, that the area of the pericardium uncovered by pleura was either absent or reduced to a minimum. In this observation I am supported by Hamernjk and Nuhn, who state that normally there is not any space in front of the pericardium through which paracentesis could be performed without wounding the pleura. Henle states that the good results obtained from paracentesis are at variance with this view, but that percussion bears it out, as in deep inspiration the left lung goes to the left border of the sternum in front of the heart. It appears probable to me that in some cases the pleura is actually wounded in paracentesis of the pericardium, but that no bad results ensues from the small puncture, and that in others the distension of the pericardium displaces the pleura laterally, and thus the disease makes way for the operation; this is the more probable, as the connection of the pleura with the lateral wall of the pericardium is firm, while its attachment to the chest wall opposite the lower part of the body of the sternum is particularly lax.

CASE OF DOUBLE OBTURATOR HERNIA.

By JOHN BARTON, F.R.C.S.;
Demonstrator of Anatomy, Royal College of Surgeons, Ireland.

[Read in the Section of Anatomy and Physiology, March 15, 1889.]

Cases of hernia in the obturator canal are not often met with, and I therefore think this case that I met with in the dissecting-room of the Carmichael College this Session worth bringing before this Section.

The subject was an old man, and, like most of the recorded cases, the hernia was discovered accidentally. The student, when drawing the small intestines out of the pelvis, was surprised to find that they were held down, and it required some force to pull them out. He then saw two openings in the wall of the pelvis, and called my attention to the matter.

There is a hernial sac in each obturator canal, that on the right side being slightly the larger; the sacs admit the last point of the index finger easily, being about $\frac{3}{4}$ inch in depth.

On the right side the obturator vessels and nerve are below, and to the outer side of the opening; on the left the nerve is below and the vessels above, springing from the deep epigastric.

On dissecting the thigh it is seen that the sac has not come through either the ext. obt. mus. or the strong fascia covering it, so that there could not be the least evidence of the existence of the hernia to be found by examining the thigh. The very stroug fascia that covers the femoral surface of the obtur. ext. mus. would seem to me to be some explanation of the small size of the hernia usually met with at this opening.

This case differs from most of the recorded cases in three particulars—

1st. Met with in a male; almost all the recorded cases are females.

2nd. The hernia being double; there is one similar case in Holmes' System of Surgery.

3rd. The existence of an obturator hernia with an irregular obt. art.

This affection has been known for a long time. One of the earliest articles on the subject that I have been able to find is "Garengeot sur plusieurs hernies singulières meni," *de l'Academ de Chir.*, t. I., 1761. Seven cases are here mentioned—one produced by a fall coming down stairs was reduced by manipulation, and one was successfully operated on, part of the sac being cut off.

There is a very accurate account of a dissection of this hernia by Jules Cloquet, *Patholog. Chirg.*, page 107, 1831:—A female subject was brought into the dissecting-room with a large tumour in the thigh, which, on dissection, was found to be an obturator hernia.

Sir A. Cooper, on crural hernia, gives a plate of a specimen in St. Thomas's Hospital, found on making a *post-mortem* on a woman, and he says it is the only specimen he knew of.

There is a very full account of this form of hernia in Birkett's article on hernia in Holmes' System of Surgery. Twenty-five cases are alluded to; fourteen of these were met with on dissection. He mentions that the Fallopian tube and part of the urinary bladder have been found in obturator hernia, also that requently only part of this calibre of the intestine is in the sac.

There are two specimens in the R. I. C. S. Museum, one presented by Mr. Kirby, and the other by Mr. Croly.

CASE OF ABNORMAL POSITION OF LARGE INTESTINE.

By JOHN BARTON, F.R.C.S.;
Demonstrator of Anatomy, Royal College of Surgeons, Ireland.

[Read in the Section of Anatomy and Physiology, March 15, 1889.]

The following unusual position of the large intestine I met with this Session in an old male subject in the Carmichael College dissecting-room:—

The ascending and transverse colon were in their normal position, but the peritoneal attachments of the cæcum and ascending colon were very long, so that the cæcum could be put into any region of the abdomen. The splenic flexure was also normal, but when the colon descended as far as the second lumbar vertebra, immediately below where the jejunum begins, it turned across the abdomen attached to the under surface of the mesentery, to the right iliac fossa; the intestine then ran up in the right lumbar region in front of the kidney and touched the under surface of the right lobe of the liver; here a short band of adhesion attached the intestine and liver; it then turned down again to the outer side of the ascending coil into the right iliac fossa and passed into the pelvis well to its right side, and so on to the anus. The normal ascending colon was in front of these, but not in any way attached to them. Both the abnormal coils of intestine were covered by peritoneum on the front and sides, and were firmly fixed to the posterior wall of the abdomen by this membrane; there was a distinct interval between them when the peritoneum dipped down to the abdominal wall. There were no adhesions or other evidence of inflammation in the abdomen except the one small band that attached the abnormal coil to the under surface of the liver where it took the turn to descend. The inferior mesenteric artery came off as usual

and gave its colica sinistra branch to the splenic flexure; the artery then ran across the spine with abnormal part of the intestine and supplied all that portion placed at the right side. Professor Macalister met with a similar case at Cambridge last Session; he also called my attention to a case published in the *Journal of Anat. and Phys.*, Vol. V., 1871, where all the small intestines were to the left side and all the large to the right side of the abdomen.

DISSECTION OF PELVIS.

By ALFRED W. HUGHES,
Flint.

[Read in the Section of Anatomy and Physiology, March 15, 1889.]

The following dissection is supposed to be made at that stage when the student has exposed the pelvic fascia, both from its pelvic and outer aspects, and all the muscles have been removed from the front of the pubis:—

The deep dissection of the perinæum (viz., the triangular ligament and structures in relation to it) which is usually performed previously to the commencement of the dissection of the abdomen, should be postponed, and the dissector should carefully preserve these deep structures until the lower limbs have been detached and all the muscles removed from the front of the pubis.

The structures on the dorsum of the penis should be dissected and traced back to the suspensory ligament, which should be defined and then carefully divided—taking care not to injure the above structures—viz., the dorsal vessels and nerves of the penis, which can now be traced back to the triangular ligament, the attachments and position of which can readily be made out, together with all the structures piercing it.

The pudic artery, which has already been seen lying in the outer wall of the ischio-rectal fossa, should next be traced forwards, and its relation shown to the two layers of the triangular ligament, as well as its continuity with the dorsal artery of the penis.

The student should next remove the pubis with a saw, cutting vertically and removing all the piece of bone which lies internal to the spine of the pubis, the anterior part of the pelvic fascia, with the pubo-prostatic ligaments (which have been previously exposed while dissecting the pelvic fascia), and beneath them will be

seen the anterior fibres of the levator ani passing down to the prostate and forming the levator prostatæ.

By removing a little fascia and areolar tissue, the prostatic plexus of veins is exposed, to which the dorsal vein of the penis can be traced. The prostate and membranous portion of the urethra should next be dissected, and their relation to the pubic arch and triangular ligament can be well shown by passing a staff along the urethra into the bladder.

The urethra should next be opened on its upper surface, passing the knife through the anterior part of the prostate, and continuing the incision into the anterior wall of the bladder, thus exposing the whole length of the urethra and cavity of the bladder, which can be examined *in situ*.

This incision would be facilitated by having a staff grooved on its concavity.

The base of the bladder, with the vesiculæ seminales and vasa deferentia, together with the posterior surface of the prostate, could next be examined by cutting through the anterior fibres of the levator ani and turning back the rectum according to the usual method.

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